THE LANCET.

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COURSE OF

LECTURES ON THE BLOOD,

AND ON

THE CHANGES WHICH IT UNDERGOES DURING DISEASE,

Delivered at the College of France in 1837-8,

BY

M. MAGENDIE.

LECTURE XXII.

Anomalies in the results of experiments.—Cadaveric phenomena are not to be taken for post-mortem appearances.—Effusions into serous cavities; causes which produce them.—Transformation of one species of albumen into another.—Composition of pseudo-membranes.—Fluids which contain albumen.—Experiments with substances which act on the albumen.—Nutritive qualities of gelatine.—Can fibrine be transformed into albumen?—Different effects produced when albumen is injected into veins and arteries.

GENTLEMEN:—Exactly a fortnight ago a circumstance of peculiar nature, and one which it is important you should be made acquainted with, occurred. It struck me that it would be useful to ascertain whether it were possible, by injecting an acidulated liquid into the veins of an animal whose blood had been artificially deprived of the faculty of solidifying, to restore it the coagulable property. I had already learned that blood, liquefied by admixture with subcarbonate of soda in a glass vessel, recovers the power of forming into a clot, when a small quantity of dilute sulphuric acid is poured upon it. But whether the same phenomenon would occur in the living vascular system I was, of course, unable to determine *à priori*. The animal I selected for the experiment was a dog, into whose veins I had already injected twenty grammes of subcarbonate of soda, on three different occasions; and what is no little extraordinary, the animal appeared to be in better health after the third than after the first injection. In consequence of the flourishing state of his health, there was a certain precaution necessary to be taken before commencing the injection,—one which should never be omitted by those who wish to come at positive results in therapeutics,—I mean the ascertaining, at the moment any remedy is administered, whether the morbid state supposed to exist, be really present. Many a drug has owed its vogue and pretended efficaciousness to errors which the habit of observing can alone ensure us from committing. You try a new mode of treatment, your patient recovers: are you quite sure that the supposed complaint really existed. There is not an old woman without her formula against rabies; and many intelligent and conscientious persons will tell you of individuals who, after having been bitten by mad dogs, made use of certain drugs and recovered. Recovered from what, if the dogs were not diseased?

I consequently made a small opening in the jugular vein; the blood which escaped was evidently changed in point of colour, but scarcely had a minute elapsed when it formed into a well-organised coagulum. See how necessary it was to make this preliminary examination. Had I omitted it, I should of course have fallen into the error of believing that I had discovered the means of destroying and restoring coagulability; of, as it were, killing an animal by one chemical agent, and recalling him to life by another.

I next introduced an additional dose of twenty grammes of the subcarbonate, dissolved in two-thirds of a pound of distilled water, into the animal's veins; but even after this injection, making in all eighty grammes of the salt, the blood coagulated almost instantly after having been drawn. I was perfectly sure of the substance employed, because the injection of twenty-five grammes of it proved fatal to another dog in half an hour; the only difference of practically important nature between the two animals being that the last alluded to was much less strong and of smaller size than the other. The disparity of size was, however, far from being sufficient to account for the different results following the injections. I made a further trial with the blood of the surviving animal by pouring a small quantity of the alkaline solution on the portion

I had removed from the vessels, and found that now everything went on as usual, the blood being perfectly liquefied by the admixture.

Well, Gentlemen, I have now to inform you that this animal died yesterday, and that I am still unable to account for the singular phenomenon I have just described. It has been suggested to me that it might depend on the existing *medical constitution*; but I confess that I have no great faith in the intervention of an influence of the kind. A certain medicine appears to succeed to-day, and in fifteen days hence it will fail when exhibited under the same circumstances; some persons would not hesitate to attribute the failure to the state of the *medical constitution*. But this, and similar methods of evading contradictory facts, are mere *ignes fatui*; with their aid we have explanations for everything, and need never fear being caught in a difficulty.

The autopsy showed that the animal died with all the signs of disordered pulmonary circulation; the lungs were heavier and more dense than is natural; and their parenchyma was generally œdematous. At the time the chest was opened, the pleuræ did not contain the least trace of effusion; but on examining them eighteen hours after, we found a certain quantity of fluid blood in their cavity. This effusion must have been the result of a simple cadaveric phenomenon, for the tissue of the lung had not been cut into, nor any large-sized vessel opened. Do not allow the practical consequences flowing from this fact to escape you; it is evident that as our hospital autopsies are not made until twenty-four hours after death, certain lesions then most distinctly visible may be mere post-mortem phenomena.

Here is another case calculated to throw light on the history of effusions into the serous cavities. I yesterday injected a small quantity of bile into the pleura of this dog, and a violent *inflammation* immediately declared itself. The membrane, or rather the subjacent vascular rete, is extremely red, and its sensibility greatly augmented. The laminæ of the pleura may always be detached, for they remain unaffected; the stratum of capillary vessels underneath is the real seat of the injection. We read in books descriptions of the *inflammation* of the arachnoid, of its redness, and the arborisation of its vessels; but in such accounts there is both physiological and anatomical error. Neither the arachnoid nor pleura are traversed by blood-vessels; white fluids only penetrate into their substance. But to return to my experiment, the contact of the bile caused the minute vessels to contract, and so oppose the course of the blood in their interior; the consequent stoppage of the circulation caused the extravasation of serum. We can, by injecting any irritating substance into the serous cavities, determine similar effusions; this is even one of the best ways of procuring fibrine dissolved in the serum, and separated from the globules. You are aware that it is difficult enough to separate the fibrine from the other elements of the blood; and that by the ordinary processes it is obtained in mixture with the globules and colouring matter. On the contrary, under the circumstances to which I now allude, it escapes alone, in solution, in the serum; and once out of the vessels it promptly forms into masses, and may be easily obtained in the shape of filaments and lamellæ.

Hence it suffices to obliterate the capillary vessels in order to produce effusion either of the blood in substance, or of some of its elements. If you wish to observe the phenomenon still more distinctly, you have only to perform the experiment on large animals, such as the horse. Give this animal an artificial pleurisy, and quarts of fluid will be exhaled on the surface of the pleura; the fluid will subsequently allow the fibrine it holds in suspension to separate from it, not in a sort of confused crystallisation, but in determinate forms, referrible to a common type of organisation. The serosity is of a citron or fawn colour, and is subject to great variety in point of the proportion it bears to the false membranes formed along with it. These false membranes first present themselves as an opaque, caseiform matter, suspended in the serosity, or spread in layers on the pleura. If the disease advance towards cure, the effused liquid is seen to be gradually absorbed; the lung expands and is separated from the thoracic parietes by the false membranes alone, which are found united into an amorphous stratum, subdivided by soft and tremulous laminæ. Reddish striæ soon appear in their centre, which are nothing more than the rudiments of blood-vessels. These are at first irregular, but soon assume a cylindrical form, and pass through a variety of gradations, until they are finally transformed into true tubes. Before the vascular tunics are organised, the blood appears to be contained in canals perforated in the interior of the fibrinous laminæ. These laminæ become elongated by the movements of the thorax, and stretch from the costal to the pulmonary pleura, ceasing to interfere with the process of respiration. The accidental tissue gradually augments in consistence, until it acquires greater powers of resistance even than the circumjacent parts; it is now definitively organised.

It is a curious fact that the irritating liquid is completely absorbed before any morbid exhalation takes place on the surface to which it had been applied. There is no bitter taste in the fluid effused in consequence of the introduction of bile into the pleura.

At our last meeting we commenced the physiological study of the albumen of the serum, and I informed you that considerable differences were discoverable between this substance as found in the blood, and in the egg of birds. This distinction, which is confirmed by our experiments, is of the greatest importance; for it appears, nay, it is certain, that the albumen of egg cannot, as might have at first been supposed to be the case, perform the functions of that which belongs to the serosity. Nevertheless I shall this moment have occasion to call your attention to a curious phenomenon which I think I have observed: I use these doubting expressions designedly, because the opinion I am about to state is founded on what must be considered only a commencement of an experimental inquiry. Here is the fact: I injected the albumen of four eggs into the veins of an animal since I last saw you, and it would appear that this albumen has changed its nature, and become transformed into albumen identical in character with that wherewith it was mixed. For the present I am unwilling to say more on the subject; for you must feel, Gentlemen, as well as I, that before giving out such an occurrence as real, our convictions ought to be founded on the most indubitable facts.

I have already, in the course of these lectures, made known to you such points of distinction as we have been enabled to trace between the solidification of the fibrine and the albumen of the blood: I shall now, therefore, simply bring to your recollection the spontaneous character of the coagulation of the former of those substances. I must, at the same time, inform you that when the fibrine passes from the fluid to the solid state in the living animal, it always carries with it, in the process of organisation, a certain quantity of albumen. This is especially remarkable in the formation of false membranes, and, indeed, in a variety of other instances. Examine, for example, the liquid exhaled on the surface of cicatrices, which subsequently, by its solidification, forms the pellicle that covers them; treat it by the appropriate tests, and you will see that it is composed of albumen and fibrine. The same is true of the pseudo-membranes of which we have just been speaking; in fact, every new formation of organised nature is constituted at once by the two elements.

In other cases the albumen is found in an isolated condition. If you leave some pus at rest, its component elements soon separate; an albuminous fluid goes to the top, and a sediment, more or less solid, is found at the bottom. Purulent matter is, in truth, formed of globules and albumen, mixed with different salts of the blood; and *apropos* of this I may mention to you that the origin of the globules of pus has of late given rise to numerous investigations. Among others, I have myself made some experiments on the subject, which have, at least, furnished the following result:—The serum contains, as I have just said, a certain proportion of albumen; I submitted some pus to the action of a pretty strong heat, and a flocculent precipitation of albumen took place, but the globules neither disappeared, nor underwent any modification, whence the necessary conclusion that they cannot be composed of albumen. If this negative character has not resolved the question of their nature, it has at all events advanced the inquiry an important step.

There are a considerable number of fluids in the economy which contain albumen, and even in larger proportion than the serum. The ovarian vesicles, for example, are filled with a viscid humour, which is yellowish and limpid, and resembles the albumen of egg. It would be an interesting subject of inquiry for the comparative anatomist to determine the relations which possibly exist between the albumen of the human ovary and that of the same organ in oviparous animals. Numerous pathological cases induce me to believe that the ovule is in the mammalia filled almost solely with albumen. I remember to have repeatedly seen the ovules, which dilate for the propagation of the species, hardened and coagulated by heat.

On the other hand, certain liquids, which appear almost wholly composed of albumen, really contain scarcely any of that principle. Such, for example, is the fluid I have named *cephalo-rachidian*, which fills the cavities and invests the exterior of the brain and spinal marrow. Still we may state in general terms, with truth, that a certain quantity of the substance in question will be found in all the great accumulations of fluid produced in the economy, whether their existence be the result of a physiological or a pathological process. Cysts almost always contain some, and in the tumours of that species developed in the ovaries a considerable proportion is always found.

Again, ulcerated surfaces have been known to throw off albumen. I have at this moment under my care two young persons affected with lumbar abscess; suppuration has ceased for some time, but has been replaced by the discharge of a viscid matter: I sent some of it to M. Pelouze, and that gentleman ascertained it to be formed of pure albumen. There are cases, too, in which the urine contains notable quantities of this principle; you are all of you acquainted with the affection termed *albuminous nephritis*, and the characteristic phenomenon on which its diagnosis is founded. It is useless, however, to enumerate all the liquids which neither in the normal nor abnormal state contain albumen; and I shall, therefore, at once proceed with the experiments commenced in my last lecture.

Sulphate of lime, dissolved in water, has

solidified the albumen with which it was mixed; we must ascertain whether well-water, which contains a large proportion of that salt, will furnish the same result. The fact we observe may, to a certain extent, explain the natural dislike we have for selenitous water; it incommodes many people not a little, though there are others who make habitual use of it without suffering inconvenience.

The *subcarbonate of iron*, a salt employed in large doses in anemia, chlorosis, &c., has communicated its orange-yellow colour to the albumen.

Arsenious acid, one of our most violent poisons, has formed a coagulum.

The other substances which you perceive in these glass vessels have not produced any very evident effect. These are the *hydrosulphate of potass*, the *bicarbonate of soda*, *mannite*, *lime-water*, *lactic* and *phosphoric acids*. *Citric acid* itself, though it exercises a very deleterious influence on fibrine, has not had any appreciable effect on albumen.

You recollect that in my last lecture I examined at some length the differential characters of the albumen of egg and of serum; and in connection with this subject I must return a little more fully than I did just now to the extraordinary results of the introduction of the albumen of egg into the circulation of an animal. I took the whites of four eggs, and strained them through a linen cloth, in order to ascertain that they were pure, and then slowly introduced them with a syringe into the jugular vein of the animal you see on the table. Almost immediately after the operation he was seized with vomiting, an occurrence which I confess I know not how to explain. During the succeeding forty-eight hours the animal remained in a tolerably tranquil state; the first symptoms seemed to have completely passed off at the end of that time, and I then injected the white of two more eggs. On this occasion there was no return of the vomiting; and on the following day I repeated the operation with the quantity of albumen last employed; the animal died almost immediately after. It is just to infer from this experiment that the introduction of albumen into the circulation may produce disastrous consequences.

But the most surprising part was that the albumen of the eggs actually changed its nature, as it would appear, on being mingled with the blood,—losing the characters of white-of-egg-albumen, and assuming those of the same principle as it exists in the serum. Some blood was drawn from the animal a few minutes after the injection, and conducted itself in the ordinary manner; that is, it separated into two parts, the one liquid, the other solid. But its serum, when treated with potass, far from forming a gelatinous mass, of opaque appearance, as you saw occur with the albumen of egg the other day, remained perfectly fluid. This is a pretty strong proof that the white of egg has lost its distinctive properties by being introduced into the circulation. Here is another equally decisive: I now take some of the serum of the animal's blood, and solidify it by pouring on it a few drops of acetic acid; I next heat it over the spirit-lamp, and you see that it immediately resumes the liquid form. Remember, too, that these experiments are made on serum which was extracted only twenty minutes after the injection of the albumen, and yet not a trace of the latter is to be discovered.

I have, at different periods, entered on researches respecting the nutritive qualities of gelatine; qualities which, in spite of the prolonged investigation of which they have been the subject, are still far from being thoroughly understood. In the course of my inquiries I fed animals on hard-boiled whites of eggs, and remarked from the first that the diet, in spite of the animals' hunger, was anything but palateable to them; they ate it with evident repugnance. They lost flesh rapidly, and in every instance where the animal was submitted to the diet for a sufficiently long time it died. Now, the result of experiment, combined with the dislike of animals for such food (an effect of the instinct that causes them to avoid whatever may prove injurious to them), argues against the alimentary property of albumen. The subject is an important one, but the present is not the proper time to investigate it.

You remember that on a former occasion we found that injections of serum into the veins rendered the blood incoagulable; it would be well to ascertain whether albumen would have the same effect on blood removed from the body. In this vessel is contained a mixture of albumen, blood and water, in the proportion of one centilitre of each; a very distinct coagulum has formed, but it has not fallen to the bottom, because as the albumen changed the density of the serum, the clot became comparatively lighter, and consequently rose to the top. In this second vessel I placed one centilitre of albumen, one of blood, and five of water; and here, too, coagulation has taken place. When the proportion of albumen is raised to three centilitres, those of water and blood remaining the same as in the last experiment, a coagulum still forms, as the contents of this third vessel show you; but the serum appears to be modified, and has become extremely viscid, as it occasionally does in certain forms of disease.

These facts, Gentlemen, throw no light on the cause of this animal's death (the one who died from the injection of whites of eggs into its veins); whether the blood was liquefied or not, can only be decided by the autopsy, to which we now proceed. The very first incision that I make shows that

the blood is in a fluid state,—a fact which indicates the effect of this injection to be same as that of serum. As the blood is liquid, we must necessarily find the lungs engorged. I remove the walls of the thorax; those organs, you perceive, are far from being in a normal state. Their cells are distended with black blood; the left organ is infiltrated in a very remarkable manner: it is impossible for an animal's respiration to continue for any length of time with disorders such as these existing in its lungs. There is another morbid appearance which must not be forgotten, I mean this scirrhous mass containing a great number of softened tubercles in its centre: this is an unusual condition in animals, and may have had some share in the production of the phenomena we have witnessed.

As the animal survived several days after the first injection, the intestinal canal, in all probability, will present some pathological condition in relation with the morbid state of the blood; and, just as I had conceived would be the case, you see that the follicles are much developed, and form eminences underneath the tunics that invest them.

There are some physiologists of opinion that fibrine is nothing more than modified albumen; for my part, I think it would be difficult, not to say impossible, to establish such identity between these two substances by well-authenticated facts. M. Denis, of Commercy, respecting whose interesting labours I have already spoken to you in the present course, was among the number of those who embraced this opinion, with the most thorough conviction of its correctness. The principal argument he adduced in its favour was, that when fibrine is treated with nitrate of potassa it liquefies, and then acquires all the properties of albumen, among others, of becoming solid by heat. Such an experiment as this was undoubtedly of a nature to lead to the conclusion that the transformation of fibrine into albumen was really possible, for it appeared exceedingly improbable, considering the simplicity of the process, that any error could have crept into the announcements respecting its issue, nevertheless I fear that this has been the case.

The fact, when generally made known, created quite a sensation in its way, so ready is the world to receive everything with favour that possesses the character of novelty. M. Denis came and described it to me; but as I am one of those who like to see things with my own eyes, and not with those of others, I begged he would repeat his experiments in my presence. The trial took place in this theatre, several of you being present at the time; unfortunately the experimentalist did not succeed in producing any of the results announced. I should be grieved if what I now say discouraged in any degree those persons, who endeavour, by conscientious researches, to throw light on questions so utterly obscure as those with which we are now engaged; but I should be well pleased if they had the effect of guarding them against that species of mistaken enthusiasm which has, in a multitude of instances, betrayed the most eminent men into serious errors. Be all this as it may, I by no means wish to affirm that albumen and fibrine are not one and the same substance; but if such be the truth, it must be proved in a different manner from what has yet been done, and I am of opinion that organic chemistry is not yet sufficiently advanced for the purpose.

I told you, Gentlemen, that the qualities of the albumen differed in different animals, and proved it also. You remember that that of eggs, when treated with potass, formed into a solid, gelatinous mass, rendered more opaque by heat; whereas the serum of human blood, treated by the same agent, did not coagulate even with the additional agency of caloric.

Here is a very curious fact which substantiates the result of our experiment on the conversion of different species of albumen into each other. Before the lecture I put some human serum in a vessel, with one-tenth of its volume of albumen of egg; this I did with the view of ascertaining whether the result would be the same as in the circulating system of an animal; and however extraordinary it may appear it is no less true, that here, also, the albumen of egg has disappeared, and that the serum treated with potass and heated, as is now done by my assistant, does not coagulate as the albumen of birds would do.

Continuing our investigation of the properties of the albumen existing in the egg, I injected the whites of five eggs, diluted with five times their volume of water, into the veins of a dog. The solution marked four degrees by the areometer, and, consequently, possessed a certain share of viscosity. Notwithstanding this, the animal suffered no inconvenience from the experiment; the functions continue in a normal state, and it is probable will so continue. This is certainly not the kind of result we should have looked for, judging from the issue of former experiments; but this contradictory fact only serves to teach us how utterly ignorant we are of the physiology of the blood, even where we fancy we are most thoroughly well informed. Here is a specimen of the animal's blood; it has coagulated perfectly well, and contains but a small proportional quantity of serum. This serum gives no evidence, by any unusual viscidity, of having had albumen added to it. I now submit it to the action of the two chief reagents, which disclose the presence of albumen; I pour a small quantity of potass on it; this produces no solidification, nor does the heat of the spirit-lamp coagu-

late it either. What, then, had become of the albumen that was injected? The certain fact is, that it has disappeared, but how, or by what series of transformations, the present state of our knowledge by no means enables us to declare.

Now, as the jugular was the vein whereby the injection was introduced in the preceding experiment, the fluid must have passed directly through the right ventricles into the lungs and, consequently, have traversed the minute capillary tubes of those organs without causing any obstruction in their interior. But we know that if we had employed any other material endowed with a certain share of viscosity, such as oil, gum, &c., the pulmonary capillaries would have been obstructed inevitably. Numerous experiments have taught us that such substances, though innocent in themselves, cause death by their mechanical effects when injected into the vessels.

It follows, then, that there must be some intimate relation between the albumen of the blood and the capillary vessels, whereof the diameter does not exceed the one-hundred-and-twentieth, or even one-hundred-and-fiftieth part of a millimetre. In physical experiments on the flow of liquids, we find that pure water either will not pass at all, or at least with great difficulty, through very minute tubes, but that when a little albumen is added to the water no difficulty is any longer encountered in making it traverse the same canals. M. Poiseuille came to a similar conclusion through his ingenious hæmostatic researches.

Be this as it may, in order to examine the question in all points of view, I proceed to vary the experiment in such manner as to ascertain whether a substance, which traversed the lung without injuring that organ in any respect, will, in like manner, pass through the vessels of the brain without interfering with the due performance of its functions.

With this view I shall introduce the injection by an artery instead of a vein, and the carotid appears to be the best fitted for the operation. I shall make use of the animal who has already had the albumen introduced by the jugular, for as he has suffered no apparent inconvenience therefrom I conceive that he may be looked on as perfectly healthy, and that the results of the experiment may be trusted to. I now introduce the point of the syringe into the carotid; the instrument holds about four ounces of a very viscid solution of albumen, as you will find if you examine what remains of it in this vessel. I push the piston gently; the animal appears to suffer some painful sensation; this is evident from his struggles. His state of anguish grows greater; I fear I shall be obliged to put an end to the experiment, the animal is so extremely ill at ease.

It would appear to result from this experiment, Gentlemen, that the introduction of a liquid by the arteries is much more dangerous than by the veins, for I have scarcely been able to inject a drachm of the albuminous fluid on this occasion. Besides this, the contractile force of the heart was powerful enough to raise up the piston, while I was endeavouring to push it down, and some blood has consequently made its way into the instrument. The animal is to all appearance dead; the few drops of his blood, which I now allow to flow, coagulate instantly. There is still some slight arterial pulsation; but it cannot long go on. The fact is altogether a most extraordinary one, but I trust the autopsy will explain it.

I have already made several experiments with dextrine, one of the elements of starch, intended to exemplify the relation borne by the globules of that substance to the capillary vessels in point of size. My present object is to examine another property of this substance. I have here a solution of dextrine, of about the same viscosity and consistence of albumen; I shall inject it into the veins of a dog, and see if its effects are the same as those of that substance: the liquid marks five degrees by the areometer.

The vein is laid bare; I introduce the point of the instrument, which contains about two ounces of fluid; the operation is terminated, and the animal gives no sign of suffering. On applying my ear to the thorax I find the respiratory murmur pure.

Let us next introduce a small quantity of the same liquid by the carotid. You see that when I do this, no apparent effect seems to follow. These facts respecting albumen and dextrine, although they appear to us to be contradictory, are certainly not so in reality; our ignorance of all the intimate phenomena that occur in these experiments is no doubt the cause of the apparent contradictions we have just observed. Of this you may rest assured, that by continuing these experiments, and by varying them in numerous ways, we shall, in the end, find that these facts, instead of being reciprocally subversive, in truth lend each other firm support.

TREATMENT OF RHEUMATISM
WITH
HYDRIODATE OF POTASH.

To the Editor of THE LANCET.

SIR:—If you think the accompanying cases of acute rheumatism, treated by hydriodate of potash, possessed of sufficient interest for publication, your insertion of them in THE LANCET will oblige yours, &c.,

C. D. MACKAY, M.D.

Farnsfield, Nottinghamshire,
Feb. 22, 1839.

December 29, 1836. A. F., a farm servant, aged 17, a stout healthy young woman, after exposure to severe cold, was seized with acute pain in her joints, which was much aggravated in consequence of being sent home (seven miles) in an open cart, while the snow was of such depth as to make her travel very slowly. I saw her late in the evening; she had then been ill for six days. She complained of excruciating pain in her knees, ankles, shoulders, and wrists; the wrists and ankles were much swollen and shining; she could not bear the slightest movement, even the pressure of my finger in feeling her pulse caused acute pain; pulse 90; tongue furred; bowels confined; no appetite. I took six ounces of blood from her arm, and gave the following powder, which I directed to be repeated in the morning, and ordered a strict antiphlogistic regimen:—

Calomel, 3 grains;
Jalap, 6 grains.

On the following morning I commenced with the hydriodate of potash,—

Hydriodate of potash, 1 scruple;
Water, 6 ounces. Take an ounce thrice a day. Repeat the powder night and morning as yesterday.

Jan. 1, 1837. Pulse 90; tongue furred; bowels freely moved; pain and swelling rather increased.—Increase the hydriodate to fifteen grains in the day; repeat the powder night and morning.

3. In all respects much as last report.—Increase the hydriodate to twenty grains in the day; omit the powders.

7. Pulse 86; bowels free; the swelling not much altered, but the skin not so tense and shining, and the pain greatly diminished.—Decrease the hydriodate to fifteen grains in the day.

13. Improving in all respects; pulse 80; bowels free; tongue less furred; swelling and pain diminished.—Decrease to ten grains in the day.

16. Great improvement; swelling and pain much diminished; can move her shoulders, knees, and ankles without pain; the swelling and pain of wrists still continues.—Decrease the hydriodate to seven grains and a half in the day; which she continued until the 24th of January, when she was convalescent.

She returned to service in the beginning of February. The weather was very severe during the whole time of her illness, and her father's cottage being cold and uncomfortable, rather retarded her recovery.

Case 2.—April 12, 1837.—Mary T., ætat. 25, a cook, unmarried. On Sunday last, an intensely cold day, she walked fast to church, about a mile, and afterwards felt very much chilled; the catamenia, which are generally profuse, were then present, but ceased on that night. I saw her on Wednesday; she then complained of cold and shivering, and "pain all over her;" pulse 76; bowels confined; no pain nor swelling at the joints.—I prescribed a dose of calomel and jalap to be taken at bedtime, and a saline aperient in the morning.

13. Worse; pulse 80; tongue furred; bowels well moved by the medicine.—V.S. to ten ounces, and repeat the powder at bedtime, and the saline draught in the morning.

15. Has continued worse; pulse 88; tongue furred; has been a good deal purged; no appetite, and has had very little sleep; pain in the shoulders and wrists; slight swelling of the wrists; does not complain of the "pain all over her."—Omit the purgatives, and take hydriodate of potass, five grains, four times in the day.

16. The wrists much swollen, hot, and shining; cannot suffer them to be touched; pain in shoulders, elbows, and ankles.—Continue the hydriodate, twenty grains in the day.

18. Pulse 86; tongue furred; bowels free; no appetite; considerable thirst; pain rather less and swelling not increased.—Take only fifteen grains.

20. The catamenia reappeared yesterday and are copious; slept well; pain much abated, and swelling diminished; feels "much easier and more comfortable."

22. Catamenia have ceased; scarcely any pain in the joints, and swelling much less; bowels free; appetite better; tongue clean; pulse 56!—Omit the medicine.

23. Continues to improve; pulse 56.

25. No pain and very little swelling; considerable perspiration during the night; appetite better; no thirst; pulse 70.

On the 27th she complained of a slight pain in her shoulder, and I directed her to take five grains of the hydriodate, which she continued for a few days, and did not again experience any return of the pain. Her pulse remained at 70—72, which seemed to be its natural state.

Case 3.—July 12, 1837. J. W., an elderly man, a horsebreaker, and of dissipated habits; has been much subject to rheumatism; has been ill for a week, and has taken purgatives without any benefit; pulse 96; tongue thickly furred; bowels free; thirst; skin very hot; urine thick and muddy; cannot move his shoulders without great pain, and both wrists are much swollen and very painful; I saw him late in the evening, and ordered,—

Calomel, 5 grains;
Jalap, 6 grains;
Scammony, 6 grains. A powder, to be taken at once; and to begin in the morning with hydriodate of potash, fifteen grains in the day.

13. Has been purged copiously; no alteration in other respects.

14. Pain and swelling of wrists increased

at his urgent request I took four ounces of blood from his arm.

15. Pulse 96; tongue thickly coated; skin hot; bowels confined; urine dark and muddy; considerable perspiration during the night; pain and swelling much as yesterday; much thirst and sensation of constriction about the pharynx.—Repeat purgative powder; decrease the hydriodate to ten grains in the day.

16. Improved; pulse 90; tongue coated; bowels freely moved; urine muddy and dark; great perspiration in the night; thirst less, and sense of constriction gone; pain much less in wrists.

18. Continued improvement; pulse 90; great perspiration at night; thirst, and a slight sense of constriction about the pharynx; bowels not moved since the 16th.—Repeat purgative powder; decrease the hydriodate to six grains in the day.

24. Has continued the mixture as last ordered (six grains per day); pulse 76; tongue clean at the point and edges, but still coated in the centre; bowels regular; urine almost natural; skin cool; considerable perspiration at night; swelling and pain of wrists quite gone; still some pain in left shoulder.

26. Continues improving; appetite returning; to decrease the hydriodate to six grains per day, which he continued until the 4th August, when he was convalescent.

Case 4.—Dec. 21, 1834. S. M., ætat. 24, waggoner at a farm; has been ill four days; he first felt an acute pain in his left hip, and soon afterwards in his knees and ankles; his feet and ankles are very much swollen, hot and shining; the left foot pits on pressure; no pain in wrists or elbows; slight pain in right shoulder; has been taking purgatives, and observed a strict antiphlogistic regimen for three days; cannot breathe freely, for he "feels pain and tightness about his heart;" pulse 104; tongue furred and white at the root and centre, but red and moist at the point and edges. I saw him late at night; I ordered him two calomel pills immediately, and a saline draught early in the morning, and then to begin with the hydriodate of potash, twelve grains in the day.

23. Sleeps very little; pulse 104; tongue white, but not so much furred; bowels free; considerable perspiration; swelling and pain of ankles as before; knees swollen and painful, and pain in both shoulders; neither pain nor swelling of wrists.—Continue hydriodate of potass.

25. Better; pulse 86; tongue white; bowels free; slept tolerably; perspiration at night; swelling and pain of ankles rather less.—Continue hydriodate of potass.

26. Feels "much better;" pulse 70; tongue white, but not furred; perspiration not so copious; no pain in shoulders; less pain in ankles, and swelling decreased; very little swelling or pain in knees; no thirst; appetite tolerable.—Continue hydriodate of potass.

27. Pulse 72; tongue whitish; no perspiration; bowels free; no pain in shoulders or knees; ankles less swollen, and pain nearly gone; feels "quite well," and refused to take any more medicine.

He soon afterwards exposed himself to rain, and had a slight attack, but which was subdued in a few days.

Case 5.—February 4, 1838. William S., ætat. 15, a "farmer's lad," has been employed during the severe frost in cutting turnips for sheep, in an open field, and has felt ill some time before he gave up work, which he did on 29th January; has been taking purgatives since that day. Pulse 88; tongue white and coated; bowels freed by medicine; face much flushed and hot, and eyes red and suffused; urine thick, and of a pale brick colour, depositing much sediment; no appetite; much thirst; intense pain in knees and ankles, left elbow and right hand, the third and fourth fingers of which are quite immoveable and very painful. He says that his knees and ankles have been more swollen a few days ago than they are at present; since yesterday he has felt "much pain and tightness about his stomach when he draws his breath.—To have the hydriodate of potass, ten grains per day, and to-morrow morning to have a purgative of calomel and jalap.

Feb. 6. Pulse 76; tongue coated; bowels freely moved; much thirst; no appetite; pain considerably worse; cannot move either arm even to take drink; wrists and shoulders very hot, but not much swollen.—Repeat purgative powder; continue the hydriodate of potass.

7. Pulse 65; tongue less coated; bowels freed twice; considerable perspiration during the night; face less flushed; slept tolerably; urine much as on the 4th; not so much pain in ankles, and he can move the left arm.—Continue hydriodate, ten grains per day.

8. Pulse 64; tongue clean; bowels not moved since last report; slept tolerably; slight perspiration; can move the left arm tolerably; right arm still very stiff, and painful at elbow and shoulder; not so much pain in ankles.—Purgative powder as before; continue hydriodate of potass.

10. Pulse 60; tongue natural; bowels free; urine of its natural appearance; slight perspiration; no thirst; appetite much better; can move the fingers of right hand, but not the arm; left knee very painful.—Continue hydriodate of potass.

14. Pulse 66; tongue natural; very slight perspiration; slept well; good appetite; no thirst; can move both arms without stiffness or pain, and the pain is entirely

gone from his ankles; very slight pain in knees.—Continue hydriodate of potass.

17. Has been up every day since the 13th, and now feels no pain except a slight one in his knees when he walks; pulse 64; tongue natural; bowels regular; sleeps well; appetite good; no thirst.—He continued the hydriodate (six grains in the day) until the 23rd, when he ceased, and he had no return of the complaint.

ON

THE COMPOSITION OF MOULDING TABLETS FOR FRACTURES, &c.

By ALFRED SMEE, *Dresser at St. Bartholomew's Hospital.*

THE importance of a substance that can be moulded accurately to any part of the body, at a moment's notice, must be admitted by every member of the medical profession; yet many difficulties attend the formation of a composition which shall, at the period of its application, be so yielding and soft that it may take an accurate cast of any part, and when dry shall still retain the form given to it, and become sufficiently hard to resist external impressions, and devoid of brittleness and much flexibility; and further difficulties present themselves when the capability of its being quickly dried is required; the advantage of lightness and cheapness would also be a great desideratum.

As I had frequently noticed that the combination of gum arabic and whiting, when dry, possessed great hardness and toughness, and was yet so free from brittleness that it could scarcely be pounded in a mortar, I was determined to ascertain how far it would answer to make tablets which might be used to form extemporaneous splints.

For this purpose a piece of coarse sheeting was copiously brushed over on one surface with a thick solution of gum, after which it was covered with a composition made by rubbing whiting with mucilage, continually adding the powder until the whole was of the consistence of a thick paste; a second piece of sheeting was now rubbed over on one side with the solution of gum, and the moistened side applied upon the composition with which the piece of sheeting had been covered, and we thus had two layers of sheeting with an intervening layer of the composition of mucilage and whiting, forming a thickness which might be increased or diminished as strength or lightness is desired. The whole was then dried, and formed a tablet about the thickness of slight pasteboard.

This experiment succeeded beyond my most sanguine expectations, for whilst the tablet remained dry it was exceedingly hard, but when sponged over with a little warm water became so yielding that by moulding it with the fingers a cast could be taken of any part of the body. The hand and knuckles were defined with great accuracy, and I succeeded by a little management in taking a cast of the greater part of the face.

It is sometimes advisable not to allow the substance to dry upon the part on which it is moulded; but after the depressions and elevations have been traced with the fingers it should be carefully removed and partially dried before the fire, and as soon as the texture is sufficiently dry to retain its shape, it may be placed near a stove, or even on the hob of a grate, without fear of being corrugated, or becoming otherwise deformed. In many cases, however, this drying is quite unnecessary, it being requisite only to envelop the moist tablet with a bandage. A cast thus taken is extremely hard and tenacious, so that, when not much thicker than a wafer, it may be struck violently and repeatedly against any hard substance, and not be destroyed. It possessed but slight flexibility, and after having been bent, it returned to its previous form, showing considerable elasticity. It was neither liable to be torn nor broken; and, lastly, it possessed the advantage of lightness, combined with durability.

Whilst in search for a moulding substance, I thought it advisable to try various compositions, in order that the best might be selected, but none appeared so excellent as that last described.

A composition of powdered starch and gum was spread upon linen, wetted with the solution, as in the first instance, when it afforded a good and firm tablet, but perhaps not equal to the first.

A paste made with gum and flour formed a good composition, but in all cases where flour is used there is a liability to more or less contraction. A mixture of plaster of Paris and gum dried very speedily, but was apt to crack, and did not wear well.

Compositions of white of egg, with the same substances to thicken it as were used to thicken the solution of gum, were next tried, the linen cloth being first smeared over with the albumen, but none were found to answer; and it was singular that the mixture of the sulphate of lime and the white of egg had so little firmness that it fell to powder when dry.

Boiled glue and whiting formed a hard tablet inferior to, but slightly cheaper than, that first described, and its unpleasant smell would prevent its use except in hospital or military practice.

Similar compositions of flour-paste were found utterly useless, having neither consistence, cohesion, nor strength. The decoctions of the Iceland moss and linseed were found

also inapplicable. The preparations of dextrine were next tried, and a mixture of carbonate of lime with the solution of dextrine made a composition which answered very well. Of all these preparations, and many others that were tried, few were applicable; and none in all respects equal to the composition of gum and whiting, both of which substances are always easily obtained, and have the additional advantage of cheapness.

The solution of gum which was found most eligible contained ten or twelve ounces of gum arabic to the pint of water.

As far as regards the nature and texture of the cloth, it is to be remarked that linen is stronger than cotton, and less liable to be torn, and therefore to be preferred. Of the various kinds of linen none moulds so perfectly as moderately coarse old sheeting; for when the tablets were made of finer Irish linen they were very inferior in this respect.

The application of these tablets is rather extensive; they may be used with great advantage for all fractures of the metacarpal bones; also for those of the forearm, or even of the humerus. When the humerus is fractured, the method which has been adopted is, to cut a piece of paper somewhat into the shape of the required splint; it should cover a portion of the pectoralis major, and extend as high as the bend of the neck, and include the whole of the scapula; from this broad plate a piece descends to the bend of the elbow, and should be sufficiently wide to cover about two-thirds of the outer part of the arm. The paper is then placed on one of the prepared tablets, which is cut to a similar shape. The piece, thus prepared, is moistened until it becomes perfectly soft, and it is then moulded on the arm and neck. From the general shape of these parts there will be found a superfluity of substance about the deltoid, which must be pinched up and turned down, so as to force a fold over the other part. The splint, then, may be in a degree dried, and its inner surface lined with lint. The whole is to be enveloped in a starched roller.*

This mode of proceeding may appear tedious, but it is a source of much comfort to the patient, for whilst the upper arm is enveloped in this hard case, so that it is quite immoveable, the forearm and hand may be left loose, and the patient may, in some degree, enjoy the use of them. The benefit of this mode of treating fractures is not confined to the patient only, it lessens also the labour of the surgeon; for when the injured limb is once put up in this manner it requires no further attention for days, weeks, or even till the cure is accomplished. Its application to chronic diseases of the joints will be found particularly useful. In these cases two lateral splints are to be formed, and enveloped in a starch roller.

It is hardly necessary to add, that in fractures of the lower jaw it must prove a valuable auxiliary. Great, however, as these advantages may be, perhaps they are trifling in comparison with the importance of its application to simple fractures of the leg. The mode of treating at St. Bartholomew's Hospital has been for some months the method first adopted by Mr. John Lawrence, of Brighton. His plan was to form two strong splints on either side of the injured leg by successive layers of pieces of bandage, united together by white of egg and flour. Now, as far as his method is concerned, it requires no improvement, as durability, strength, and an accurate cast are obtained by this mode of proceeding, and the numerous cases which have been treated by it at the hospital show its complete success. By using the tablets formed of gum and whiting, upon the same plan as that of Mr. John Lawrence, a great saving of the surgeon's time is effected, and equal firmness and durability obtained. The mode in which I have made splints for the leg is first to obtain the exact shape by drawing a piece of sheeting or paper round the leg, and marking the part which corresponds to the tibia for the whole length of the leg, and continuing the line on the foot to the extent it may be considered necessary to cover.* By this means it is apparent that the exact size of the limb is obtained; but as the leg is to be enclosed by two splints, it becomes necessary to divide the cloth into two, which will give an exact pattern of either splint. These splints are to be moistened and moulded, and after being first lined with lint or leather, the whole is to be enveloped by a roller soaked in boiled starch.

This composition of gum and whiting has answered perfectly in the cases in which it has been tried, and splints made of it are, perhaps, superior to the splints made with flour and white of egg, because when dry they preserve accurately the shape of the limb, and do not at all corrugate, which all compositions of flour are liable at times to do. Fractures of the patella are treated in a similar way, a splint being placed on either side of the knee, extending from the centre of the thigh to about the centre of the leg. The patella is not to be covered with these splints, but a gap corresponding to its shape left, and the two pieces or

* The roller is merely soaked in boiled starch, and wound up in the usual manner before it is applied.

* Either splint should overlap the heel and under surface of the foot in cases where they are used immediately after the accident, but where this application is delayed this is of no importance.

splints are not to meet accurately at any part, but an interval is to be left of about three-quarters of an inch, or an inch, during their whole extent. In enveloping these splints for fractures they are not to be applied when there is much inflammation or swelling, but the part should be allowed first to get into a perfectly quiet state, or leeches, cold water, and poultices should be applied, if necessary, to effect this object. In general a delay of a week, ten days, or even sometimes three weeks, is required; but in some favourable cases there is no occasion to wait, and the splints may be applied with safety and advantage on the second or third day after the accident. This mode has also been adopted in favourable cases of compound fracture; but most surgeons are agreed never to cover these wounds with concealing bandages. It is not for me to expatiate upon the advantages with which this method of treating fractures is attended, for that belongs rather to Mr. John Lawrence, as the first adopter of the principle, but the fixing of the bones more firmly and securely than can be accomplished by any other method; the prevention of loss of health, by enabling the patients to walk on the fourth or fifth day after receiving the accident, and permitting them to be removed to a situation more healthy and airy; the prevention of stiff joints, and the more speedy and final uniting of the bone, are advantages too great to be passed over unmentioned.

These advantages are likely to be enjoyed by a greater number when the time required for the first application of the splints is diminished, and the objection is removed of allowing the limb to remain without bandages during the time required for the drying of the splints; the tablets which I have described possess these additional advantages, and with these superior cheapness is also conjoined.

Bank of England, Feb. 9, 1839.

THE INFLUENCE OF THE IMAGINATION ON THE FŒTUS IN UTERO.

To the Editor of THE LANCET.

SIR:—The extent to which many are disposed to carry the belief that the fœtus is affected by external impressions upon the mother, or by her imagination, is too notorious to be questioned. Some may find a reason for this in the supposed ignorance of the parties; but strange and striking confirmations of the fact present themselves among the most intelligent, and among those, too, who least sanction the belief of such a doctrine.

At a very early period of the world the doctrine was believed in, an illustration of which we have in the account of Jacob's stratagem. Hippocrates was a supporter of the doctrine; for we find that he maintained, in defence of a noble female who had borne a coloured child out of the course of nature, that a picture, which hung in her apartment, of an Ethiopian, was sufficient to account for the appearance of the child. Galen adopted the same opinion. It is recorded, too, that Fabius Quintilian preserved the honour of a female who had borne a negro, the parents being white, by boldly asserting that she had, during pregnancy, taken great pleasure in examining the picture of a negro which was placed in her chamber. Heliodorus' interesting novel of the "Loves of Theagenes and Characlea," is hinged on the fact of the latter being born white, of Ethiopian parents. Dr. William Hunter entertained an opinion, founded on strict observation for a long course of years, altogether different from those above noticed. A very puzzling case, touching the question, once occurred under the doctor's own eye; yet he would not be a convert, and most ungallantly nourished a suspicion of the fidelity of his fair patient, who had borne as heir to a title, to a native of Britain, a Mulatto.

With the breeders of cattle of the present time, the opinion upon which Jacob of old seemed to have acted, universally prevails. I have notes of some remarkable confirmations of the peculiar influence under notice occurring in the brute creation, which, however, I shall not at present detail.

There are already numerous cases on record which prove that external impressions upon the mother, and that her emotions affect the child in utero. External deformities and marks have been more particularly noticed; but may not the brain, as well as the skin or form, be influenced? May not those organs, which are only developed in the independent state of life, be set in the order of play in the dependent and inconscious state of being by the mother's imagination or state of mind? Here is a subject for inquiry; but I shall not enter upon it, contenting myself with relating a few cases which have come under my own observation. I may add that I have no doubt whatever of the truth of the statements which I received in connection with each case.

1. A girl approaching an interesting age, who had become extremely anxious to have a mark upon her face removed, consulted me. The mark is a fair profile of a bird, black, and covered with downy hair, and situated on the right cheek, immediately under the eye, the head being towards the temple, the body placed over the superior part of the cheek, and the tail, somewhat short, occupying the greater part of one

side of the nose. The girl's mother indulged me by giving the whole history of the case, the father confirming the statement as she proceeded. It was shortly this:—One day in the early stage of pregnancy, while busy cleaning the house, the mother by accident came in contact with a cage hanging from the ceiling, in which was a linnet. The concussion, together with the fluttering of the bird, gave her a great start, from which she had scarcely recovered, when the little captive, finding its prison-door open, escaped, and in its flight struck the woman's face on the corresponding identical spot to where the mark is on the girl, who, by the additional start, was quite stunned. The occurrence took deep root in the woman's mind, and till the day the child was born the circumstance constantly recurred to her recollection with its full complement of dreads.

2. While extracting a tooth for a young woman my attention was drawn to a strange appearance of the tongue, gums, and lining membrane of the cheek of the side in which the tooth was situated. The parts appeared tumefied and ecchymosed, as if recently injured. The mother of the young person perceiving that I took particular notice of the state of the mouth, removed the covering from her daughter's neck, and showed me a tumour, of a black colour, situated under the angle of the jaw of the same side, and told me the following story:—A few weeks after marriage, when on a visit to some of her husband's relations, at dinner she was presented with a piece of black pudding. It seems that knives and forks were not used on this occasion, a circumstance which made the young wife a little uneasy. She experienced a strange confusion, and was sensible of blushing at her awkwardness, but unwilling to offer any objection to the fashion of the family, accepted the pudding and began to eat. A small piece had been masticated, but she could not swallow it. In her confusion a second piece was tried, but, in the act of breaking it off with the teeth, the pudding slipped from her fingers and, in the effort to catch it, it was compressed against the upper part of the neck, between the ear and chin. Her confusion was increased manifold, and for a long time she could not forget the circumstance. Without any assistance from the imagination, the external tumour may be said to represent a piece of black-pudding very faithfully, and the likeness of the masticated part is quite as faithfully pourtrayed by the parts within the mouth.

3. At a farm-house in the country, I had frequently, in passing, seen a fine healthy boy running about minus a hand; and it occurred to me, naturally enough, that the little urchin had been the subject of an operation. In the course of time I was made acquainted with the fact that the child had been born without the hand. The mother told me that during early pregnancy she had, for the first time, seen a poor deformed idiot, an habitual way-side annoyance, who was nearly powerless of one side of his body, and had the hand of the same side bent in such a manner upon the arm as to give it the appearance of a stump. This last circumstance shocked her more than anything else, and she felt as if a cold current passed through her body at the moment, accompanied with a feeling of horror, which frequently returned on recollecting the "unsightly object."

4. While stethoscoping a young female I observed a peculiar mark over the right scapula. On inquiring what had caused it, she told me that she was born with the mark. Some time after, I had an opportunity of questioning the mother on the subject, when I got the following, what she called "lee-liketale." A neighbour, an old woman, and she were sitting at work near an open window on a summer evening, when her husband, from without, jokingly let drop under her dress a *bunch* of black currants. She sprung to her feet in great alarm, and cried out, desiring to know what it was, experiencing at the moment an indescribable sensation, but was immediately satisfied, and had the fruit taken out. The old woman instantly reproved the husband for such levity, considering his wife's state, giving him to understand that many had cause to regret such-like folly. From this time it was held to be matter of certainty that the child would be marked. The mark is very much like a cluster of black currants. I was told that it is only during the time the fruit is ripening that the mark is full and distinct! I could add at least half-a-dozen cases of the same description, the history of which I know.

D. R. RANKIN.

Carluke, Feb. 18 1839.

ANATOMY AND PHYSIOLOGY OF THE TEETH.

To the Editor of THE LANCET.

SIR:—Allow me to make a few observations on a paper read at the last meeting of the Medical and Chirurgical Society, as reported in THE LANCET, of February 2nd, on the Persistent Nature of the Dental Capsule, &c., by Alex. Nasmyth. Having myself paid some considerable attention to the subject of that author's investigations, more especially since the publication, in Müller's *Archiven der Physiologie*, of the researches of Retzius into the minute structure of the dental bone, and having, by careful examination of accurately prepared sections, convinced myself of the correctness

of the views entertained by that author, Purkinge, Müller, and Professor Owen, in our own country, I cannot hesitate to express my dissent from the doctrine laid down in the paper referred to; and I cannot but regard it as one of those cases of over-strained analogy to which the present zeal in physiological and anatomical research has given rise in more than a few instances; as one of those *ignes fatui* by which, in matters of scientific inquiry, we are so apt to be dazzled and led into error. With all due respect for the zeal and perseverance in the investigations which have engaged their attention, by which the eminent physiologists above named are distinguished, and, with an entire appreciation of the value of the discoveries which have resulted from those labours, it will, I trust, be no sacrilege to express a distrust of *some* of the conclusions to which they have arrived. In so far as they have unravelled the delicate and complex structure of the teeth, and that in a manner at once more skilful and satisfactory than has yet been done, we should yield them all the honour of having made a great advance in the anatomical history of these organs. Yet, at the same time, while, on the one hand, guarding against that scepticism and jealousy of new views which are disgraceful alike to philosophy and humanity, we should, I apprehend, entertain a wholesome caution against the fascinations of mere novelty. That the structure of the dental bone consists of tubes commencing in the parietes of the internal cavity of the tooth (cavitas pulpæ), and that these tubes which have been termed by Professor Owen calcigerous tubes, from the circumstance of their containing the earthy salts, which enter so largely into the composition of this structure, run in every direction from the centre to the surface, where a free anastomosis takes place with their terminal branches, no one who has been at the pains to investigate the subject in the manner which Retzius has pointed out, can have any doubt. In a carefully prepared longitudinal section, also, the membrane which the same author has pointed out as being interposed between the bone and the enamel, may be distinctly seen folded in between the fibres of the latter structure, which have been described as hexagonal crystals, resting with one extremity on the membrane just described, which binds them up into clusters and thus gives them protection and support: and this, I have no doubt, is the appearance to which Mr. N. alludes when he states, that he "was induced to attend to this subject from having observed detached portions of membrane floating from preparations of human teeth which had been subjected to the action of acid." And it is equally surprising and unaccountable that, not content with so simple and rational an explanation, he should have arrived at the extraordinary conclusion that the enamel is invested with a membrane external to itself. As a dentist, and familiar with these organs, he must have felt, in his daily practice, that such a notion was so surrounded with difficulties as to be altogether untenable.

It is a fact perfectly well known to every person of the slightest observation, that the enamel, so far from being covered with a protecting membrane, is itself an inorganic covering for the protection of the organised bone, and that it undergoes abrasion from the first moment that the tooth enters upon its function. That teeth, the eruption of which has not advanced normally, may be seen covered with the attenuated and half-absorbed capsule, there can be no doubt; but that this is the case with those which have made their appearance in the ordinary and natural manner is, I am convinced, as incapable of demonstration as that a child continues enveloped in its membranes through life. Of this Mr. N. seems aware, and he therefore takes refuge in an ample saving clause: he observes:—"This capsular covering continues throughout life, *except it be worn away by attrition.*" Now, we may be quite sure that this is the case in every instance in which a tooth is normally developed, and, therefore, what has been said of an old law is true of this proposition,—that it is like a sieve through which everything may be allowed to escape,—and the proposition would be little less absurd, that the child continues enveloped in its membranes through life except they be ruptured at birth.

I will not pursue the analogy further; the life of a tooth may be divided into two stages, the formative and the functional; and it is obvious that the capsule belongs to the first, and is removed when it ceases. What, then, is the value of the "physiological and pathological observations" deduced from this *discovery?* or must they not rather be considered an useless expenditure of mind? To suppose that this view "will serve to reconcile many conflicting opinions concerning the vitality of the teeth," or, "that the phenomena of disease are due to the *vital powers* of the *different portions* (?) of the persistent capsule," is to attribute effects to a cause which has no existence. That morbid sensibility of the teeth which is usually described as their being set on edge, is unquestionably to be referred to the periosteum; and those who are at all accustomed to operations on these organs, know perfectly well that no sensation is produced on the enamel under any circumstances, and that it is not until this structure has been removed and the denuded bone is acted upon that pain is experienced. I am, Sir, your obedient servant,

VERAX.

PRESERVATION OF THE TEETH BY MECHANICAL MEANS.

As long as appearances are preserved by the presence of the front teeth, the loss of the side teeth, or grinders, is frequently viewed as a matter of little importance. This is a great error, for it is the presence of the grinders which keeps the mouth sufficiently open to prevent the front teeth from coming in contact during mastication. When, therefore, the grinders are lost, and their places are not supplied by artificial means, the front teeth soon become either worn away, or loosened, and pushed from their sockets.

Few persons are aware of the cause of losing their front teeth. Some attribute the loss to a local defect in the teeth themselves, and others to constitutional causes. They seldom or never reflect that the front teeth were not intended, and hence, are not adapted for masticating purposes, which invariably destroy them. The entire process of mastication belongs to the grinders, and the only function which the front teeth are intended to perform is comprised in the word "cutting."

When even a single grinder is lost, the whole of the teeth on that side of the jaw are weakened by the breach which it leaves, and which deprives them of mutual lateral support, and renders them apt to be pushed from their proper perpendicular position towards the opening, by the opposite teeth. But this is not all, for as soon as a tooth in one jaw loses its masticating opponent in the other, it begins to protrude from its socket, loosen, and ultimately falls out. So that the loss of one tooth, by rendering its opponent in the other jaw useless, amounts to the loss of two.

When the teeth remaining for mastication are too few in number, to sustain the force of the jaws, they are soon destroyed, by being either forced into their sockets, so as to produce disease and absorption, or crushed and broken, occasioning grievous pain, followed by the total loss of such teeth. The front teeth being unprotected, through the loss of the grinders, are soon destroyed in the way before described; and, proper mastication being now impossible, derangement of the digestive functions ensues, attended by privation of comfort and loss of health.

Fortunately, the whole of this mischief may be remedied, and the greater part of it prevented, by the timely adoption of artificial teeth. When any of the side teeth are lost, their places should be immediately supplied by properly constructed artificial teeth, so as to prevent the others from slanting towards the opening left by the lost teeth. Artificial teeth, by meeting the natural teeth in the opposite jaw, preserve them by preventing their protruding from their sockets; and, mastication being thus restored, health is recovered and preserved. The artificial teeth, by preventing the jaws from shutting too close, preserve the front teeth, which would otherwise be destroyed, by meeting together in the process of mastication.

The object in supplying artificial teeth has hitherto been too generally confined to mere show, at the expence of the other teeth; whereas, the whole aim should be to preserve the remaining teeth, and restore mastication, which secures comfort and health. When many teeth are lost, all tampering with the remainder, in the shape of picking and filling, can only increase suffering, and hasten the loss of teeth so tampered with. The operator must be perfectly aware of this; but as the continual suffering produces constant visits, and unmerited fees to the picker, these unhappy patients are the most profitable to him. Such practitioners, instead of pointing out the proper artist capable of affording the only real relief, strenuously advise their patient dupes against the adoption of preservative pieces of artificial teeth; for mere tooth pickers, being incapable of supplying this remedy themselves, know that delusion would be dispelled, and their malpractices exposed, if their victims fell into the hands of a competent mechanical artist.

PHILOCRUST.

THE EXCITO-MOTORY SYSTEM.

LETTER FROM MR. CARPENTER.

To the Editor of THE LANCET.

SIR:—As Dr. Marshall Hall has not thought proper to give any *direct* reply to my letter of Dec. 31, I had not intended to have trespassed again on your pages in reference to the matter which it concerned. There is, however, in your last Number, a remark, *quasi* editorial, which shows such a complete misapprehension of the point at issue, that I must claim once more the privilege of setting myself right with your readers.

I imagined that I had stated, in the letter to which I have referred, in as explicit a manner as possible, that I then regarded, and always had regarded, the doctrine of the *distinct system of excitor and motor nerves* as completely Dr. M. Hall's own, whatever might be its other merits. That he should have thought it necessary to write to M. Flourens for a disclaimer of this, in opposition to what I had stated of the opinions of the latter physiologist, shows a kind of misapprehension on Dr. Hall's part, which, were I disposed to make use of his epithets, I might with much show of justice designate

as wilful and malicious. But I do not think so uncharitably of Dr. Hall; and rather believe that his zeal to establish his favourite opinions, and his own property in them, has in some instances (and this among the rest) somewhat got the better of his judgment.

I again repeat that in stating, as I formerly did ("British and Foreign Medical Review," April, 1838, p. 532) that "the general results set forth by M. Flourens appear to us to contain nearly all that Dr. M. Hall can be said to have demonstrated," I did *not* include the doctrine of the distinct system of excitor and motor nerves, a part of the article in question being directed to show that it *could not* be regarded as a *demonstrated* truth. If Dr. M. Hall will procure from M. Flourens an assurance that he has not, from the time of Sir C. Bell's discoveries, held the opinion that the impressions conveyed to the spinal chord by the efferent nerves may, in virtue of its "excitability," produce sympathetic actions through the motor nerves, without sensation and volition, I shall be quite ready to admit that I have mistaken the tendency of M. F.'s previous writings, which appeared to myself, as well as to others, obviously to lead to this "general result." But I shall never admit that I have *wilfully* misstated his opinions with any desire to detract from Dr. Hall's merits; because my conscience perfectly acquits me of any such intention.

You intimate, Sir, that M. Flourens' concurrence with Dr. M. Hall's doctrines is a strong argument in their favour. But you do *not* mention how many physiologists of at least equal eminence,—Professor Müller and Dr. Alison, for example,—withhold their assent from them—(I am alluding, of course, to Dr. H's *peculiar* doctrine, that of the *distinct system*.)—So that if authority is to guide us, I apprehend that its balance will be still against the theory.

But, Sir, this is a question which is to be decided by evidence, and not by authority. In the article which has formed the basis of the present discussion, I pointed out that whilst the doctrine did not seem necessary to explain phenomena, there was nothing to prevent its being established, could additional evidence be produced in its favour. My verdict, in short, was one of *not proven*. I have recently been engaged in searching for such additional evidence, with, I hope, no incapacity to receive or appreciate its value; and Dr. M. Hall will, I doubt not, be surprised at my candour when I say that the results of my inquiries are, in my own mind, decidedly *favourable* to his peculiar doctrines. I should have much pleasure in communicating them to him, did I suppose that he could set any value upon them; but so long as he regards me capable of wilfully perverting truth for the sake of depreciating his merits, it is obvious that no evidence adduced by me in his favour can be relied on by him. Whenever he is disposed to admit that I have an equal desire with himself for the establishment of TRUTH, from whatever source it may come, I shall be quite ready to unite with him in the search for it. I am, Sir, your obedient servant,

WILLIAM B. CARPENTER.

February 21, 1839.

REDUCTION OF ARSENIC.

To the Editor of THE LANCET.

SIR:—I should, before this, have noticed Dr. Wilks's reply in THE LANCET for the 19th January, to my criticisms on his alleged improvement of Mr. Marsh's apparatus, but I waited for leisure to perform some experiments on the subject, that what I have to state might be on the evidence of my senses, as well as on the conviction of its truth by reasoning.

Dr. Wilks does not attempt to defend his assertion, that in Mr. Marsh's method the arseniuretted hydrogen is rapidly dispersed, carrying with it a great deal of the metallic arsenic unreduced, consequently I conclude he admits my proofs to the contrary. In answer to the danger of explosions which I said there would be, from the admixture of hydrogen with the air of the bottle and tube, Dr. W. says:—"It is only necessary to wait till the gas comes over briskly before it is inflamed, and then all danger of explosion is prevented." Now, the last time I tried his proposed improvement, on putting a light to the end of the tube when the gas was coming over briskly, the bottle and tube were blown into minute pieces with a loud explosion, and scattered in all parts of the room. I fortunately escaped with a few slight cuts of the face, but I strongly caution others against the same experiment.

To my statement that where only a minute quantity of arsenic was present, it would be gone with the first portion of hydrogen, before the gas came over sufficiently briskly to be ignited, Dr. W. replies, that "pure hydrogen being lighter than arseniuretted hydrogen, the former escapes first, a fact of which any one may convince himself by watching the experiment." I have convinced myself of the very contrary by watching the experiment, and have found what I first stated to be true; besides, that pure hydrogen should escape first is at variance with the laws of chemistry. When there is any oxide or salt of arsenic in the solution under experiment, the metal begins to combine with the nascent hydrogen from the very commencement, and the arseniuretted hydrogen arises from the fluid along with the free hydrogen gas, and both are, in the bottle, in a state of intimate mixture, and gases when once mixed do not again sepa-

rate; even oxygen and hydrogen, when once shaken together, do not separate; and between these gases there is a difference of specific gravity three times greater than between hydrogen and arseniuretted hydrogen.

Dr. Wilks says, in his reply, that he was aware that Mr. Marsh's apparatus was a syphon and not a bottle, and, also, that he readily admits the syphon to be well adapted for generating the gas; but, alas! he proposes to substitute his own bent tube, the size of a quill, for the jet of small bore. What takes place then? Precisely what might be expected; when the tap is turned, and the light applied, the pressure of the fluid forces out the gas all at once, and for a second there is a large flame outside the tube. If the quantity of arsenic is considerable there may be a slight deposit of arsenious acid, or of brown arsenuret of hydrogen, outside the tube, and a slight garlic odour may, perchance, be perceived, but there is no coating of metallic arsenic. I must repeat, then, what I said in my former letter, that a phial and pipe-stopper, which Mr. Marsh tells us may be used on an emergency, are infinitely better than Dr. Wilks's so-called improvement in any form.

Mr. Scoffern, who, it seems, has seen a part of this correspondence, but not seen my letter, describes this bottle and pipe-stopper in THE LANCET of last week, as a form of apparatus he has been in the habit of seeing used; but he erroneously attributes its suggestion to Mr. Pereira, instead of Mr. Marsh. He says that some practical experience enables him to affirm that the arsenic is not dissipated whilst we are waiting for the escape of the atmospheric air from the bottle; but I presume he is not speaking of Dr. Wilks's wide tube, but of the pipe-stopper, concerning which no one has ever made the assertion. But I notice Mr. Scoffern's letter principally to tell him that in his future "practical experience" he must not be content with holding a piece of mica or glass over the flame, and collecting arsenious acid, but he must hold a piece of glass (or a piece of broken porcelain or china will do) in contact with the flame, and collect a crust of metallic arsenic; this is the grand test of the presence of arsenic, and it is on this that the great value of Mr. Marsh's discovery depends. Having got metallic arsenic he may, if he pleases, collect arsenious acid, but he must not waste his gas by trying to collect this in the first instance. I remain, Sir, your obedient servant,

CANDIDUS.

February 16, 1839

APOTHECARIES' COMPANY AND ADULTERATION OF DRUGS.

AN abstract of the evidence of Dr. R. D. THOMSON, on adulterated drugs, similar to that published in our last, having appeared in the "Morning Chronicle," which seems to have been copied from the Shrewsbury paper, the following correspondence was inserted in the pages of the "Chronicle":—

ADULTERATION OF DRUGS.

To the Editor of the Morning Chronicle.

SIR:—I am desired by the Committee of Managers of the Apothecaries' Company to call your attention to an article in your paper, of the 21st ult., on the adulteration of drugs, which contained a paragraph relative to this Society. This article purports to be part of the evidence of Dr. R. D. Thomson, before the Select Committee of the House of Commons on the Poor-Law Amendment Act; but, as you will see by comparing the two passages, it is given in your paper very incorrectly, and so as seriously to reflect on the character of this Society. In the report (p. 87) Dr. Thomson's words are, "Peruvian bark is often *made* by the *drug-grinder* of charcoal, Venetian red, Carthaginian bark, lignum vitæ, and satin-wood; this mixture was *sold* about forty years ago by the Apothecaries' Company, and it was also sent to the army as Peruvian bark."

In the article alluded to in your paper, it is said, "This composition was *made* by the *Apothecaries' Company*, and was supplied to the army as Peruvian bark." On comparing these two passages it is evident that, although Dr. Thomson's statement is, from the wording of it, ambiguous, it by no means bears the construction put upon it in your paper. But in order to ascertain correctly the meaning of Dr. Thomson's statement, I called upon him and requested him to explain it, which he did in the most candid and distinct manner. He stated that the scope of this part of his evidence was to expose the frauds of the drug-grinder, who at that time often made such a preparation that he imputed no fraud to the Company but that forty or fifty years ago such a preparation had been sold at the Hall: that it had also been sent to the army as Peruvian bark by the party who then supplied the army; not by the Society, who (he was aware) had never supplied the army with medicines. So that while Dr. Thomson only meant that such a medicine had been sold by this Society, your article charges them with having made it and sent it to the army. Whether forty or fifty years ago this Society had been thus imposed on by the drug-grinder it is impossible, after such a lapse of time, to ascertain, especially as Dr

Thomson refuses to state his authority for it. I can only say that no record of such a fact appears on the books of this Society, and from the careful manner in which the business of the Hall has always been conducted, it is hardly possible that it should have occurred. For more than thirty years all the drugs have been ground at the Hall, and before that period, when the establishment was not equal to the grinding of all the drugs required, the practice was, not to send the drugs to the grinder for him to grind, but to hire his mill for their own use, and to send their own men with the drugs, who performed the process themselves, and kept them under lock and key till they were finished and removed by them to the Hall; and every preparation after it is made is examined by a committee before it is offered for sale. So that it is not likely they should have been imposed upon in the manner stated; and I am quite sure that, with their knowledge, neither this nor any other adulterated preparation has ever been sold by the Company.

As your paper has been the means of spreading the misstatement, I trust you will in justice publish this explanation, in order to correct a misstatement so erroneous and injurious to this Society. I am, Sir, your obedient servant,

H. Robinson, Treasurer to the Apothecaries' Company.

Apothecaries' Hall.

Dr. Thomson's reply; from the "Morning Chronicle" of Dec. 24:—

ADULTERATION OF DRUGS.

To the Editor of the Morning Chronicle.

Sir:—In your paper of yesterday, Mr. Robinson, Treasurer of the Apothecaries' Company, has made a statement, purporting to be an account of what occurred at an interview with me, relative to my evidence given before the Select Committee of the House of Commons on the Poor-Law Amendment Act. I beg to say that this gentleman had no permission from me to make any such statement, and further, that the account is very far from correct. Mr. Robinson called on me three times; in the first instance without any previous intimation of his object, so that he was admitted on the same terms as a patient or any other private individual. To publish a notice of what occurred at such an interview is undoubtedly a disreputable breach of common courtesy, even if it were true.

The result of the frequent calls of this gentleman was three communications made by me to him in writing, in answer to letters from him, in which I stated that I did not consider myself at liberty to interfere with evidence given before the House of Commons; but that I had no objection to compare any abstract with my original evidence, and say whether it was correct; but that further than this I could not go. My words were—

"The passage as contained in my evidence was as follows (see Medical Inquiry, p. 87): 'Peruvian bark is often made by the druggrinder of charcoal, Venetian red, Carthaginian bark, lignum vitæ, and satin-wood; this mixture was sold about forty years ago by the Apothecaries' Company, and it was also sent to the army as Peruvian bark.' The only interpretation which I can conceive this passage to bear is the affirmation of three distinct propositions."

The object in giving evidence before the Committee, I affirmed, was not to ascribe motives, but to state facts. These Mr. Robinson does not attempt to disprove; but the "wish being father to the thought," he gives an explanation which he had no authority from me to make. At the same time, I told him that I could not recognise the right of any individual, without permission of the Committee of the House of Commons, to cross-question me upon my evidence. But I was perfectly prepared to substantiate every statement which I had made, when called upon to do so by the Committee of that House. It is, therefore, not true to say that I refuse to state my authority.

There are two other passages in the abstract in the "Morning Chronicle" of the 21st ult.,* which are inaccurate. The first is, "It was his opinion that drugs adulterated as described were commonly sold to country practitioners." My words are,—"Several of these powders, to my knowledge, are administered by medical men without their knowledge of the adulteration:" referring to medical men generally. The second passage is, "A chemist had urged the witness himself to deal with him, saying, in reference to the prices, that he could supply him with drugs for paupers much cheaper." This statement does not refer to me, but to another witness. I am, Sir, yours obediently,

Robert D. Thomson.

Gower-street, Dec. 22.

It is obvious, from this communication, that Dr. Thomson, although he does not ascribe any motives whatever to the druggrinders, or to the Apothecaries' Company, the former of whom Mr. Robinson would have him libel, by no means exculpates either party from a knowledge of the adulteration. It is not, indeed, likely that if motives had been an object of inquiry sufficient proof could have been obtained to have

* The statement in the "Morning Chronicle" was copied from some country paper: but we cannot, from an examination of the paper, tell the particular one.—Editor.

admitted of a judgment of innocent being passed in this affair. It is much more probable that the monopolists of the time considered the trick a very clever imposition; although, Heaven knows, it is a very shallow one. In villainy, no doubt, it is deep enough. Those who are acquainted with the history of Rhubarb Hall, and of the condemnation of drugs by their own committees after they had found their way into druggists' shops, will receive for as much as it is worth the interesting information given by the treasurer, that all drugs before they are offered for sale are examined by a committee.

After the publication of the preceding distinct affirmation that Dr. Thomson would neither accuse nor exculpate the Company of wilful adulteration, the following stupid letter ought never to have appeared:—

To the Editor of the Morning Chronicle.

Sir:—In your paper of this day I observe a letter from Dr. R. D. Thompson, relative to my statement inserted in your paper of the 21st inst., in which he makes two charges against me.—1. "That I had no permission from him to make any such statement;" and 2. "That the account is very far from correct."

To the first charge I reply, that in my letter to him of the 12th inst. I proposed that either he himself should explain his propositions relative to this Society, or leave it to me to state them, from the conversation which I had with him.

Having no reply to this I addressed him on the 19th requesting his answer, to which I received a verbal message, "That there was no answer." I therefore considered that I was not only at liberty, but in justice to this Society bound to adopt the other alternative, and publish, myself, the explanation which he had given me. With regard to the second charge, the incorrectness of my statement, I must observe, that having had three conversations with him on this subject, I do not hesitate to affirm, that my statement is substantially correct, and I only lament that Dr. Thomson did not obviate any possibility of inaccuracy by publishing the explanation himself, which I wished him to do, and which he has hitherto declined.

I have only to add that I am prepared at any time to publish the correspondence that has passed between us on the subject, in order that the public may judge of the whole of this proceeding. I am, Sir, yours obediently,

H. Robinson, Treasurer of the Apothecaries' Company.

Apothecaries' Hall, Dec, 24.

The *honest* old treasurer's intentions are obviously very well meant towards himself and brother monopolists, but he is very thickheaded withal.

THE LANCET.

London, Saturday, March 2, 1839.

In the next Lancet, after having had about ten days experience in his office, Mr. Wakley will address some remarks to his medical brethren, relating to the new duties which he has just been called on to discharge. In the meantime, he would caution them against attaching too much credit to the *medical* portion of the reports which have appeared, or may appear, in some of the newspapers. In a short time the reporters will be more *au fait* at their business in the new department, and medical expressions and technical terms will be better understood and more accurately given. From what Mr. Wakley has already seen, he is inclined to believe that the public and the profession have suffered, throughout the whole of England, in a greater degree from the appointment of non-medical Coroners than has been conjectured even by the most zealous of the labourers who have endeavoured to remove the more glaring defects in the mode of administering the duties of the Coroner's Court.

The *Apothecaries' Company* and the *Adulteration of Drugs* are very intimately connected. The Company is an adulteration itself,—but the connection to which we wish now to advert is that of cause and effect. It appears from the remarkable facts stated in evidence by Dr. R. D. Thomson, and the correspondence between that gentleman and one of the Hall functionaries, that the *Worshipful* Company at one time sold adulterated drugs. Whether their drugs are as genuine and efficient now as they should be, it is not our intention to inquire, as we shall rest satisfied with tracing to the constitution of the Apothecaries' Company—as to a common fountain—the appalling adulterations now existing in the drug-market, and the present degraded state of pharmacy in Great

Britain. No country in the world possesses scientific chemists more industrious, talented, and successful; no country possesses a body of medical practitioners more judicious, skilful, and conscientious; yet we do not hesitate to declare that of bad, useless, pernicious trash, administered as medicine, no long-suffering people endure such quantities as the inhabitants of the United Kingdom. Extend the comparison to France, Italy, Germany, or even Russia, and it will still be to our disadvantage. Who can read the iniquitous mysteries brought to light by Dr. R. D. Thomson without starting with indignation when he finds the basest substitutions for remedies of every day's use? Is it not revolting to discover that a practitioner who thinks he is administering flowers of sulphur, is lining his unhappy patient's intestines with stucco,—that sulphate of barytes should be substituted for calomel in laryngitis or iritis, where the delay of a few hours leads to the destruction of vision, or of life,—that a dose of rhubarb cannot be given unadulterated in England,—and that even barley-meal is mixed with jalap?

It is under the Apothecaries' Act of 1815, and under the auspices of the Apothecaries' Company, that this insufferable state of things has been induced. Some short-sighted reformers have regarded the Act of 1815, and its precious bantling, the Corporation, in a favourable light. But have they reflected that that Act was based in folly, injustice, and monopoly? Did not that Act give a drug company the power of examining and controlling all the practitioners educated in England,—the power of visiting druggists-shops, — of vending medicines themselves,—and, instead of sharing the profits fairly and honestly with the Licentiates, of putting all the surplus into the pockets of the sensitive Messrs. Robinson and Co.? No wonder, indeed, that the Blackfriars Bullcalf should roar when the iniquity of the Company is brought to light. Still we say "Prick him; prick Bullcalf, till he roar again." A man, or a band of men, who enjoy a real monopoly, and sell drugs at an enormous profit, merely by means of the position which they occupy as public functionaries, without ever giving an account of their stewardship, or sharing the proceeds with their brethren, can scarcely be trusted with the care of public interests.

The London Apothecaries' Company must be considered the principal direct cause of the drug adulterations. In the first place, from their inefficient scrutiny of the druggists' shops. Their nominal visits never extend beyond the metropolis, so that upon their own admission they neglect nine-tenths of the nation, and leave them to the mercy of men who do not scruple to substitute "oak-bark, and other rubbish," for cinchona. Their examination of drugs in London is idle, and entirely nugatory. The turtle-fed masters look at and smell a few preparations, but never submit specimens to careful analysis. They have never detected one of the adulterations in the drug-market. How does Mr. Chairman Robinson meet that charge? How does he account for the fact that a Company, established, and maintaining its existence, under the condition of visiting the druggists' shops, and exposing adulterations, has allowed the base system to gain universal sway? How did it happen that the exposure of the gigantic fraud was left to the public spirit and chemical acumen of Dr. R. D. Thomson? It would look better in Mr. Robinson to answer these questions, than to write puffing letters in the "Morning Chronicle." Unfortunately, the performance of a public duty has a less direct connection with the Company's cash account, than the few lines which aroused the Hall functionary; otherwise we would ask him at his earliest convenience to bring forward *one instance* in which the visitors of the Company have detected an adulteration, or destroyed a useless drug. The dexterous Chairman—who appears to be not an unworthy *double* of the baronet of Pallmall—will, perhaps, adduce as a signal example, both of the sagacity and impartiality of the Company's visitors, that memorable instance in which they threw

articles supplied by the Company itself, into the street, and destroyed them! If we leave him in the quiet possession of this triumph, we must still ask him for an instance in which the agents of the Company have detected drugs less obviously bad, or such gross, but comparatively minor adulterations, as have been pointed out in the drug-market by Dr. THOMSON.

The Apothecaries' Company is, in the second place, the predisposing, exciting, and proximate cause of the adulterations in the drug-market, inasmuch as the existence of the Company has hitherto prevented any steps from being taken to reform the state of pharmacy. The chemists and druggists undergo no chemical examination; and the majority of them fall far below the continental *pharmaciens*, who almost invariably receive a chemical education, and often cultivate pharmaceutical science with success. Let us not be misunderstood. We have often censured the druggists who impose on the poor, in pretending to give medical advice, and we feel the more ready to admit that there are several chemists and druggists who have made themselves masters of their art in the midst of discouragements and difficulties. It would, indeed, not be possible to point out any class of Englishmen, placed in adverse circumstances, that did not present some individuals who had triumphed over those circumstances; and it will be found that there are a few chemists and druggists in England who keep as good drugs, and compound prescriptions as accurately, as the foreign *pharmaciens*. But the instances are rare, and as a class they are inferior to their foreign brethren. Now, how does it happen that the people of England get worthless, inefficient, adulterated drugs in sickness? The commerce and manufactures of the country could supply the best. And how does it happen that English chemists are the most ignorant of the class in Europe? Englishmen are not generally inferior to the men of other nations. The real solution of the two questions is, that the London Apothecaries' Company has been permitted, under a disgraceful statute, to usurp the place of a Faculty of Medicine; while it has depreciated pharmacy, and, far from endeavouring to educate and control chemists, has endeavoured only to extend the sale of its own preparations, of which the chivalrous Mr. ROBINSON and his friends realise the profits.

The degradation which the Company in Blackfriars has inflicted upon medicine is vividly depicted by Dr. MUEHRY, of Hanover,* in a work on the state of medicine in France, England, and Germany. We do not agree with all Dr. MUEHRY's strictures: and he has fallen into some misstatements which could only be pardoned in a foreigner. The *General Practitioners* of England will bear a comparison in efficiency with the physicians of any country, although every obstacle has been thrown in the way of their education, and every artifice employed to render them satisfied with the lowest grade of knowledge, as their enemies are well aware that knowledge is power, and the only real foundation of superiority. Yet the general practitioners have triumphed over every difficulty they have encountered excepting one,—the label, or libel, or license, of the rhubarb venders of Blackfriars. That source of injury we hope soon to see removed. With these preliminary qualifications, we proceed to quote Dr. MUEHRY's sketches:—

"THE APOTHECARIES

who, without the supervision of any Board, practised medicine, surgery, and midwifery, were incorporated in the year 1815, accidentally, in fact, on the occasion of the then existing Apothecaries' Company,—a mere trading body,—presenting a petition for a diminution of the duty on glass. They now unite in one Board what is excluded from the others. The Apothecaries' Company, or Society, examines the *general practitioners* of England and Wales; in other words, those medical men who combine the dealing in, and preparation of, drugs, with the practice of medicine, surgery, and midwifery. The Society also visits the apothecaries' shops. It has a *Court of Examiners*, [and such Examiners! unequalled in Eu-

* Darstellungen und Ansichten zur Vergleichung der Medizin in Frankreich, England, und Deutschland. Von Dr. A. Muehry.

rope], and the members admitted receive the title of *Licentiates*. As is natural in 'a nation of shopkeepers,' they are regarded by physicians and surgeons as of a much lower grade than themselves; but they are by far the most numerous body, and their services cost the public less, as the drugs delivered, and generally ordered by themselves, but not their visits, are paid for."

The writer goes on to say in another part:

"Physicians are so few in number, and their fees are so high, that *general practitioners* become the physicians of the poor, the middle, and even of the higher ranks, in all the slighter cases. Apothecaries do not write prescriptions; after a visit, they send medicines to the patient, and with a few recent exceptions, are paid by charges on the drugs, and not separately for their professional services. They are consequently charged with giving too much physic, from pecuniary motives; and, in fact, in cases of sickness a fresh supply is ordered to be repeated every morning. If they are surgeons they may write prescriptions. In the more important crises of disease, a physician is called in, but this occurs very seldom in the country. So a general practitioner is, sometimes, at once physician, surgeon, accoucheur, apothecary, and shopkeeper. Pharmacy is, perhaps, their feeblest side; in this department the apothecaries stand much lower than the French and Germans. Their chemical preparations are, for the most part, made at the Apothecaries Hall (!), so that little more remains for them than the mere mechanical mixture. The pharmaceutical manufactory carried on at the Hall is a capital reason for changing the present position of the apothecaries. They are eligible neither as physicians nor surgeons to hospitals. Should a young man, when he is 16 years old, become an apprentice in an apothecary's shop, during, or at the end of, five years apprenticeship, have attended the three winter, and two summer courses of lectures, and, after all, be rejected a second time at his examination, he pays no further regard to the Apothecaries' Company, for he is at liberty to commence as chemist and druggist, to vend medicines, and to practise in undisturbed impunity. Chemists and druggists are very numerous in England. At certain intervals the shops and drugs are visited with two members of the College of Physicians, but, as the drugs are purchased at the Hall, the apothecaries inspect their own drugs. The English apothecaries have gradually approached nearer to the physicians, and removed from the chemists. For physicians they have not yet acquired enough; for apothecaries (chemists and druggists) they have given up too much."

We shall not stay to correct Dr. MUEHRY's errors. He expresses the universal opinion of foreigners respecting the state of pharmacy in England. The Apothecaries' Company is regarded with unmitigated contempt wherever it is known; and part of that contempt attaches to general practitioners, in a mind like that of Dr. MUEHRY.

Of the necessity of taking some immediate steps for the reform of English pharmacy, there can be no doubt. The practitioner and the patient must no longer be deluded by adulterated drugs. Dr. SANGRADO, who bled his patients, and gave them nothing but warm water, insisted that the water should be genuine, and particularly cautioned his assistants to see that it was not spoiled in the preparation by ebullition.

The detection of the adulterations of the drug-market is the first step to improvement. Dr. R. D. THOMSON—a scientific chemist, upon whose statements we place full reliance—has, then, rendered an important public service in directing attention to the state of our therapeutic agents, by the inefficiency and uncertainty of which all the hopes of the patient, and all the skill of the practitioner, are defeated.

The root of the evil—the Blackfriars Company—must not be dealt with partially; in the general reform of the medical institutions of the country, it cannot escape. The meek Mr. ROBINSON may count upon that; and make the most of his short rule. For the rest, we have no desire now to take the Worshipful Company out of Dr. THOMSON's retort: but, if the distillation be carried much further, it is our opinion that *spiritus ammoniæ fœtidus* will be both abundant and cheap in the course of the season.

ELECTION OF MR. WAKLEY AS CORONER FOR MIDDLESEX.

ON Monday last, at four o'clock, P.M., terminated the contest for the Coronership of the county of Middlesex, in the election of Mr. WAKLEY. The state of the poll, as declared by the Sheriff, was:—

For Mr. WAKLEY 2015
Mr. ADEY 582

being a majority of *fourteen hundred and thirty-three* votes in favour of the Medical Candidate. After having taken the necessary oaths, Mr. WAKLEY addressed the assembled freeholders, but Mr. ADEY's retreat was "mute and inglorious." The Attorney Candidate did not present himself on the hustings. A great victory has thus been gained, almost, indeed, without a battle, but not without having entailed a heavy expense on the victor; for Mr. ADEY, faithful to his promise, has kept the poll open to the last day.

LIBERALITY OF THE MEDICAL AUTHORITIES IN FRANCE.

WE have frequently had occasion to praise the liberal manner in which the French hospitals, and all other medical institutions in France, are thrown open to foreigners, who are admitted to them as freely, and on the same footing as the natives of the country. An act of much higher liberality has been recently shown at Havre, in the appointment of an Englishman, Dr. Nicholas Tarral, as one of the physicians to the Havre General Hospital. We have every reason to believe that the addition which has been thus made to the medical staff of the hospital, will prove of great advantage to the sea-faring portion of the population of Havre.

ANATOMY OF CLUB-FOOT.

ALTHOUGH the malformations which are known under the popular denomination of club-foot are extremely frequent, and their treatment carried to a high degree of perfection, we possess but few accurate records of dissections of the affected limb. The following account of a case of talus, lately presented to the Royal Academy of Medicine by M. Bouvier, is therefore worthy of some attention:—

It occurred in the person of a man, who died at the age of sixty-six years in the Hôtel Dieu. The malformation had been developed in this individual when at the age of twelve months, and consisted in a forcible extension of the foot, by which the weight of the body was made to fall entirely on the heel. The angle which is formed by the axis of the foot with the leg, is sixty degrees, and the point of the foot is with great difficulty brought down so as to form a right angle. When this is done the tibialis anticus, extensor communis, and extensor proprius pollicis muscles, are thrown into a state of extreme tension. The integuments of the heel are thick and horny; those of the rest of the foot are fine and thin, showing that the heel had to bear the entire weight of the body. The os calcis is directed somewhat outwards; the whole foot also deviates outwards in a slight degree; the sole of the foot, instead of forming an arch, is nearly flat. The lateral peroneal muscles are shortened, but those which cover the back of the leg are elongated. The whole limb is remarkably wasted, the muscular fibres completely deprived of their colour, and presenting that peculiar fatty appearance which so often occurs in cases of club-foot.

The operation of dividing the tendons, which has been practised with so much success in cases of children affected with club-foot, has never, we believe, been tried on a patient far advanced in life. It was, therefore, a matter of some interest to determine how far the malformation, in the present instance, might have admitted of remedy by surgical means. The tendons of the anterior muscles were divided, and immediately the point of the foot was brought down to a right angle, the deformity almost completely disappearing.—*Bul. of the Academy*, Dec. 1838.

MEDICAL JURISPRUDENCE.

THE determination of the period since which a fire-arm may have been discharged is a point of much importance in medical jurisprudence, and evidently applicable to various cases of homicide, wounds, &c. The question has recently been examined with much care, by M. Boutigny, who has ascertained, by numerous experiments, that we can indicate very closely the period at which a fire-arm has been discharged. It may, however, be objected that as the barrel of a gun may be easily washed, all traces by which the medical jurist is guided may thus be obliterated. M. Boutigny has provided against this objection, or rather determined the characters by which it may be known whether a gun-barrel has been recently washed or not. The author has discovered that the iron of a gun-barrel does not become oxidised for a considerable time, whenever the interior of the barrel has been lined, as it were, with the residue of the combustion of powder; and even when oxidisation does take place the traces escape the naked eye, because the oxide is gradu-

ally dissolved in the acid of the sulphate of potash, or in that resulting from the oxidation of the sulphuret of potassium. Hence it follows that the wadding of the gun will present certain differences, according as the gun may or may not have been washed before having been charged.

We must refer the reader to the original article ("Ann. d'Hyg. et de Med. Legal.," January, 1839), for an account of the experiments of the author, whose conclusions only we here insert.

The wadding of a gun which has been reloaded without having been washed presents a greyish-black tinge; but if the gun have been cleaned the wadding is of an ochre or deep-reddish colour. However, when a gun has been charged immediately after having been washed, and the wadding is examined a few hours afterwards, the colour is then found to be a greenish-yellow, which passes rapidly to a brown-red, when exposed to action of the air and atmospheric moisture. If to the preceding characters we add those which are derived from the absence or presence of sulphuric acid, we may conclude to a certainty that the gun has been cleaned or not, before it has been charged. In order to render the materials which are to be submitted to the medical jurist available certain precautions must be taken by the magistrates or police authorities into whose hands the suspected arms may, in the first instance, fall. The muzzle of the gun should be closed with a paper wadding, and then covered over with some paper to which an official seal should be attached. The same precaution should be employed with respect to the lock of the gun, whether it be a flint or percussion one.—*Arch. Gen. de Med.*, Feb. 1839.

NATIONAL VACCINE ESTABLISHMENT.

REPORT FROM THE NATIONAL VACCINE ESTABLISHMENT, FEBRUARY, 1839.

To the Right Hon. Lord John Russell, Secretary of State for the Home Department.

Feb. 11, 1839.

My Lord,—The small-pox has prevailed epidemically, and with great severity, not only in England, but also in a considerable part of the continent of Europe, since our last report.

It seems, from the history of this disease, that it has recurred epidemically once in twelve or fourteen years ever since its first introduction into these islands, and always with extraordinary violence and destruction of life; so that 45,000 persons are said to have died in one of these epidemic years before inoculation was introduced, at the beginning of the last century. Since that practice was brought here the loss of life by small-pox within the bills of mortality was 5000 annually; but since vaccination has superseded inoculation the number of deaths has decreased gradually, until it amounted to only 200 in the year 1837. In the course of the year which has lately terminated (during which the small-pox prevailed epidemically) there have died 800 of this disease; not more, after all, than one-sixth of the number who died annually during the prevalence of inoculation, notwithstanding the increased population of the metropolis and its neighbourhood. Surely this implies some general protective influence, and our confidence in the efficacy of good vaccination remains unabated. We are, indeed, convinced that the indiscriminate vaccination which has been practised in this country by ignorant and unqualified persons, with but little or no regard to the condition of body of the person to be vaccinated, to the selection of the vaccine lymph, or to the progress and character of the vesicle to be formed, are to be regarded as amongst the main causes of the occasional failure of vaccination; and we are sorry to hear an anxiety expressed that a recurrence should often be made to the disease of the cow which first supplied the genuine protective matter; for, in the first place, it is not in the nature of any other communicable virus to degenerate and lose its influence; and, in the next, we have the opportunity of bearing our most ample testimony to the continuance of the efficiency of the original vaccine lymph, introduced by Dr. Jenner, through nearly a million of subjects successively, of whom many thousands have been exposed with entire impunity to small-pox in its most malignant form; and though we ourselves have taken a good opportunity more than once or twice of recruiting our stores with fresh genuine matter from the cow, yet, we think it right to discourage an indiscriminate imprudent resort to this expedient; because the animal is subject to more than one eruptive disease, and a mistake might possibly be made in the selection of the proper pustule by an inexperienced hand.

We have vaccinated, by our several appointed vaccinators, 18,659 persons this last year, and have sent out to various parts of the world 203,818 charges of lymph; the former amounting to 6241 more than have been vaccinated in the metropolis and neighbourhood in any former year, and the latter exceeding distributions of lymph from the National Institution by 79,097 charges.

(Signed) Henry Halford,
Hons. Leigh Thomas,
Thomas Watson,
Clement Hue, M.D.

MEDICAL SOCIETY OF LONDON.

Monday, February 18th, 1839.

Mr. BRYANT, President.

TREATMENT OF INFLUENZA.

DR. T. THOMSON finished the reading of his paper on influenza. The treatment of this affection could with difficulty be laid down in a systematic manner, as the plan pursued must vary with the idiosyncrasy of the patient and the complications which might be attendant on the affection. In the generality of cases, however, a mercurial purge, followed by a dose of castor oil, and repeated every second or third day, and the avoidance of local congestion, was a successful mode of proceeding. The patient should be kept in bed. Where suffocative catarrh was present, then mild tonics, such as serpentary, with small doses of ammonia, might be exhibited. Should there be crepitation in the lungs, then leeches might be employed, and if actual pneumonia were present bleeding, but to a moderate extent, was serviceable. Opium, recommended strongly by some practitioners, he had generally found injurious, as it had checked the secretions. Some conversation followed, but we have given the opinion of members on the subject in THE LANCET of two years back.

Monday, February 25th.

DEFORMITY OF THE CHEST IN INFANTS FROM DISEASE OF THE LUNGS.

Mr. G. A. REES introduced a child to the notice of the Society, labouring under a deformity of the chest, which he considered to have arisen entirely from disease of the lungs. The child was fourteen months of age, very diminutive, and much emaciated. *Auscultation.*—There is vesicular and puerile respiration in the subclavicular regions on both sides; lower down in front and behind on the right side we have bronchial respiration; while we have vesicular breathing on the left side behind, and bronchial in front, still lower down there is absence of the respiratory murmur over the lower lobes of both lungs. The belly was tumid, and the chest contracted. "The deformity consists in a depression existing between the ribs and their cartilages, in consequence of which the arched form of the front of the thorax is lost, and a channelled appearance external to the sternum, on each side, produced." "Accompanying these deformities," says Mr. Rees, in a paper which he has written upon the subject, "there is an altered movement of the ribs in breathing, and there are well-marked symptoms of lesion of the organs of respiration; the infant suffers habitually from shortness of breath and a short dry cough, when attacked by any bronchial affection not in itself of a dangerous character, bronchial irritation, for example, to which they are peculiarly liable; the usual quick breathing becomes hurried, the pulse much accelerated, the countenance assumes a livid hue, and the appearance of the child indicates a severe form of pulmonary disease."

In the case before the Society the little patient had, till within the last six months, a normal appearance of the chest, and the respiratory organs were healthy. At this time pneumonia of a subacute or chronic character, indicated by a crepitant râle, set in, and the chest had become gradually contracted and deformed to its present state. Mr. Rees has seen six other cases of this affection, in none of which were there any adhesions of the pleura; he exhibited to the Society the lung of one of these patients, with the portion of the ribs corresponding; the lung was hepatised, and the ribs fallen in in the same manner as in the patient exhibited. There were no marks of adhesions. In two of the cases which had come under his notice, the mesenteric glands were diseased, but in the other four cases the chylopoietic organs were perfectly healthy. In none was tubercular deposit present. In the child exhibited digestion appeared to be well performed, and the defecations were exceedingly healthy. Mr. Rees in his paper referred to (and which we quote, his very imperfect remarks at the Society not conveying a complete account of the matter), suggests the following explanation of the phenomena:—"The lung in chronic pneumonia becomes shrunken as well as solidified; therefore, when the diaphragm contracts in inspiration, one of two things must occur; either the descent of the muscle must bear an exact relation with the limited expansion of the lung; or the space which would result from its descent, the lung not expanding, must be filled by a temporary contraction of the walls of the chest; in the adult subject the former of these would take place; but in the infant the flexible ribs and their cartilages readily yield to the contraction of the muscle within, and to the atmospheric pressure from without, and hence the altered movement of the ribs. At a later period the deformity of the chest establishes a relation between the containing and contained parts." Mr. Rees continues:—"When the deformity is well marked, as far as I have seen, the patient never recovers; it seems as if the permanent eversion of the ribs prevented the lung from assuming its healthy state, even after the disease has been checked; where, however, the altered movements of the ribs alone exist, small doses of mercury, with the application of counter-irritants, will succeed in subduing the inflammation, and the evidence of improvement will be in the movement of the ribs becom-

ing natural during inspiration, the parietes of the chest in these cases proving a source of diagnosis as to the state of the organs within."

Dr. JOHNSON and Mr. DENDY considered that the evidences of pneumonia in the case before the Society were insufficient; changes like that observed in this patient constantly took place in the cartilaginous extremities of the cylindrical bones of scrofulous children; the deformity in the present case took place at the union of the ribs with their cartilages, and they thought it was probable that it might arise from a strumous diathesis.

Dr. T. THOMSON thought the chief disease in this case was in the chest, but he considered that there was a difference in the nature of the disease affecting the two sides; for on the right side, when the ribs were depressed, the movement took place simultaneously, as though one body only was moved, a sign he had never witnessed except as a result of pleuritic adhesion. On the left side the ribs in their movement had a more independent action one with another.

Dr. BENNETT believed that the deformity might be produced by disease of the lung independent of adhesion of the pleuræ, and that it probably depended upon some modification of inflammation peculiar to children of tender age.

WESTMINSTER MEDICAL SOCIETY.

Saturday, February 23rd, 1839.

MR. HALE THOMSON, President.

DOES OPIUM-EATING SHORTEN LIFE?—DISCUSSION ON THE CAUSES, NATURE, AND EFFECTS OF MALARIA.

THE PRESIDENT stated that a preparation of opium, called meconate of morphia, had been used by several practitioners as a substitute for tincture of opium, and Battley's liquid, with the best results. The dose was similar to that of laudanum, over which preparation it had decided advantages. The mode of preparing the meconate would be laid before the profession in the course of a week or two.

Mr. WINSLOW said that the question respecting the influence of opium on longevity was one of interest. In the case of the late Earl of Mar, who was in the habit of taking large doses of opium, the policy was disputed on this ground. The most celebrated toxicologists were examined on the trial which occurred in this case, and though none of them could adduce any facts in support of their opinions, yet, reasoning on general principles, they agreed that the use of opium did affect the duration of life.

Mr. RUTHERFORD ALCOCK thought that a reference to the mean duration of life in Turkey, where the majority of persons ate opium, would go far to solve the question. In Turkey the mean duration of life was fair.

Mr. WINSLOW said that it was thought by many persons that the acids which the Turks constantly used had some influence in preventing the ill effects of opium-eating. So fully impressed was the government of China of the evil effects of the habit in question, that it had issued edicts against it.

Dr. JOHNSON considered that the question was one of difficulty; much, of course, however, would depend on the circumstances under which the opium was taken. If taken for the relief of tic douloureux, or other very painful diseases, the use of the drug was favourable to longevity. How far its employment as an intoxicating agent affected the duration of life, could only be proved by statistical data. The average duration of life in Turkey was below that of England, but this fact could not be advanced as a proof of the injurious effects of opium-eating, inasmuch as in many other countries where this practice was not common, the mean duration of life was also much below that of this country,—this was the case in Italy and France; much, no doubt, depended on the influence of climate.

The PRESIDENT stated that the assurance offices refused to grant policies to opium-eaters.

Mr. SNOW said that those who had written on the subject described the opium-eaters of Turkey as very emaciated and miserable, even more so than habitual drunkards in other countries, consequently it was fair to conclude that the habit must shorten life. As opium-eating, like other intemperate habits, however prevalent, was still but the exception to the general rule, the average duration of life in Turkey was not a test of the effects of the habit; the true test would be the average duration of the life of opium-eaters, as compared with that of the rest of the community in Turkey.

After a few unimportant observations on this subject, the

DEBATE ON MALARIA

was resumed.

Dr. J. JOHNSON divided his observations into three heads: the sources, the effects, and the prevention of malaria. Like other morbid poisons, the exact nature or essential character of malaria was totally unknown to us; we could only judge of it from its effects. Two conditions were essentially necessary for the production or existence of malaria; these were the decomposition of vegetable or animal matter, or of both, and the presence of moisture. Malaria was, however, rendered more active and deleterious by heat and quiescence; and according to the degree in which these

existed, so would the miasma be more or less prejudicial. Thus, for instance, it was found that in the tropics when the thermometer ranged from 80 to 86, the yellow and jungle fevers were prevalent; in the colder months the less severe effects of malaria, such as ague, were observed. So in those extra tropical climates, in which the heat was occasionally intense, as in Gibraltar, Malta, the valleys of Spain and of Italy, the fevers were often as malignant as those of the tropics, and more especially when the sources of malaria were abundant. The unfortunate expedition to Walcheren in the autumn of 1809, which was intensely hot, also proved this, the fever which was then so prevalent amongst our troops being of as serious a character as any that occurs in the East or West Indies. The influence of quiescence over malaria was equally great, running waters and violent winds acting as great preventives to it. In this climate, with its canopy of clouds—its rains and winds, we were free from the more malignant results of miasma, but we suffered from it in a minor degree, and the sources whence it sprang were frequently overlooked. In London, large quantities of vegetable matter in a putrescent state were constantly being carried into the Thames, and between each tide became exposed to the influence of the sun, by which much unhealthiness was produced; but there were many other causes of miasma. Our drainage was bad; this was particularly the case on the Surrey side of the river, where the land lay much lower than on the opposite side. Buckingham Palace, which had been mentioned by Dr. Addison as so badly situated, was, in point of fact, twelve or fourteen feet above the level of the water in the park. Spring Gardens, at the other end of this piece of water, was much more unhealthy, for, during nine months of the year, the moist south-west winds were blown over the park towards it. He (Dr. J.) who had passed harmless through many countries in which malaria was most destructive, caught an ague, which lasted for many months, during his residence in the above gardens, though, perhaps, the drain of his house, which, at this period, was obstructed, might have had much influence in the production of the disease. Among other sources of malaria might be mentioned the common sewers, with their chimneys opening into every street; the decay of animal and vegetable matter in the highways; the use of water-closets in our houses, and the filthy state of our dust-holes. The effects of malaria in London were indicated to the observant physician in the "lack-lustre eye," the yellow anxious countenance; the dullness of the mental, and the languor of the bodily powers of the patient. He believed that the greatest and first impression of malaria was always made in the nervous system, producing a host of anomalous feelings and symptoms, well known to the town practitioner, but sought for in vain amongst the writings of our nosologists. The above symptoms were accompanied with wandering pain, varying in intensity from that which seems to be merely the result of a languid state of the system, to the torturing agony of neuralgia. There were two complaints in particular which had their origin in malaria, and which deserved separate notice—these were periodic neuralgia, and that which was commonly called "sick headach." The neuralgia alluded to formed a complete epitome of the ague as he had observed it on the banks of the Ganges. The pain came on at a certain hour; there was a feeling as if cold air was blowing on the eye, which was felt the moment the pain commenced; the sensation of coldness subsided; the vessels of the conjunctiva became distended, as if much inflamed; and this was shortly succeeded by a profuse lachrymation. Here were the cold, hot, and sweating stages of ague—and a perfect intermission followed. The "sick headach" was generally periodic, occurring at intervals of a week, fortnight, or month; the patient waking in the night with an intense headach, and only saved from vomiting by keeping the recumbent position. This disease was much more prevalent in the malarious parts of London than in others. The digestive organs were constantly becoming disordered by malaria, the secretions being vitiated, the appetite impaired, and the bowels irregular; these, again, gave rise to an infinity of complications. Regarding the prevention or cure of the effects of malaria, the best practitioners found that daily attention to the evacuations, the correction of disordered secretions, and the establishment of a good digestion, by means of tonics, was the best plan to pursue. Tonics, indeed, due regard being paid to the evacuations, were the sheet-anchors of the physician in these cases.

Mr. Rutherford Alcock admitted that heat might increase the effects of the malaria, but all the bad results of the poison might occur without it. During the seven years which he passed in the Peninsula, the most destructive malarious fevers occurred during the rainy seasons, in the months of November, December, January, and February, when the heat was moderate. During the summer the troops were healthy, but on the occurrence of the rain the hospitals became filled. Few countries were more liable to malaria than England, yet we did not experience much heat.

Mr. Roderick had found, in fatal cases of malarious fever, that the brain was most frequently the part affected.

Dr. Stone had observed this to be the case in many malarious patients whom he

had examined during his stay with the British Legion in Spain.

Mr. WALKER considered that one of the most prolific and destructive sources of malaria, was to be found in the state of the church-yards of the metropolis. He was inclined to attribute more mischief to the horrible state of these places than to any cause he had heard mentioned. In his neighbourhood (Drury-lane) the grave-yards were absolutely saturated with human putrescence; what effect this must have on the inhabitants of a densely-crowded neighbourhood might be easily imagined. In some places the bodies were not eighteen inches from the surface, and, in some instances, the coffins of the paupers were so thin and badly made, that a part of the corpse was exposed before its burial. Several cases had come under his care, arising entirely from malaria. One was that of a young girl who had been ill for eighteen months; she lived in a room over a slaughter-house; she was previously in good health, but on her removal to the room in question, her health and strength gradually drooped. She had no pain; tonics afforded her little assistance. It was curious that when the weather was moist she was always worse; when it was dry she was comparatively well. In the case of a man suffering from typhus now under his (Mr. W.'s) care, the bed of the patient was not eight feet from the graves in a neighbouring yard. He had collected many facts relating to the subject, which he would shortly lay before the Society.

Dr. A. THOMSON said, that although heat and moisture applied to the body produced a certain sedative effect, in many cases, instead of inducing fever, they caused rheumatism. It would appear that, during the putrefactive fermentation of animal and vegetable matter malaria was generated; but what was the particular gas forming a constituent of the malaria, which possessed the injurious power? From experiments which he had performed, he believed that the hydrogen and sulphuretted hydrogen gases were the most deleterious. In one experiment he had placed the body of a dog in a bag containing hydrogen gas, with a small portion of sulphuretted hydrogen, and though, as was known, this animal did not perspire by the skin, death quickly took place by the sedative effect produced on the cutaneous nerves. In malarious districts there was always, at some period of the day or night, a quantity of fog for some hours, and wherever this was the case, he had noticed the barometical pressure was much less than usual, indicating that there was a much larger proportion of uncombined hydrogen in the air than was generally thought. Now, where decomposition was going on, there would be a great evolution of hydrogen, and hence the source of one of the evil effects of malaria might be hinted at.

The discussion was adjourned.

ROYAL MEDICO-CHIRURGICAL SOCIETY.

STATISTICAL ACCOUNT OF CHOLERA IN THE SEAMEN'S HOSPITAL, 1832.

By GEORGE BUDD, *M.D., F.R.S., Physician to the Seamen's Hospital.*

THE chief object of this paper is to show the influence of age, and some other circumstances, on the mortality of cholera, and the liability to that disease. The author observes that there are many circumstances which render the *Dreadnought* peculiarly favourable to inquiries of this nature. The patients, who are admitted without any other restriction, are all sailors, persons similarly circumstanced, and leading the same kind of life; when, therefore, we investigate in them the influence of age on diseases, we find already eliminated the disturbing influences of sex, profession, and many other particulars, which render such an investigation extremely complicated in general hospitals; the registers of the *Dreadnought* are kept with the greatest care, and contain all the information that can be desired respecting the nation, the age, the quality, of every patient admitted, the time he has been at sea, and the vessel in which he last sailed; and, by a recent regulation, the ages of the sailors who enter the Port of London are registered at the Custom-house. The author has obtained permission to examine these registers, and has thus been enabled to ascertain the proportional number of sailors of any age, who enter the Port of London, or the proportional number of the persons of a given age who are admissible into the *Dreadnought*. After narrating a few circumstances connected with the appearance of cholera in London in 1832, the author remarks, that on the 22nd of May in that year, H.M.S. *Dover*, which had been fitted up as a cholera hospital, was placed under the management of the *Dreadnought* Committee, and stationed in the river, at Deptford; and that between this time and the 12th of January, 1833, one hundred and sixty cases were received there. He then proceeds to give, in different tables, some analysis of these cases. The first table shows the mortality in different periods of the epidemic. It appears that the mortality varied in a remarkable manner, becoming gradually less as the epidemic approached its termination. At first the number of deaths was double that of recoveries; towards the end of the epidemic, that is, after the month of September, it was only half their number. The second table

exhibits the influence of age on the mortality. The mortality was least in persons between the ages of fifteen and thirty; and in these the number of deaths was double that of recoveries; it was greatest in persons above the age of fifty; of twenty-two above this age only two recovered,—the age of these two was fifty-three; of thirteen, whose ages exceeded fifty-three, not one recovered. But advanced age is not only unfavourable to recovery in cholera, but gives a predisposition to the disease. This is shown by table 3, in which the numbers of cholera patients, within certain limits of age, are compared with the proportional numbers of sailors within the same limits of age. It appears from this table that persons between the ages of twenty and thirty, are least liable to attacks of cholera; and that above the age of thirty, the liabilities to attack continually increase with age. By means of the tables 2 and 3, Dr. Budd calculates the relative risk of losing their life incurred by persons of different ages, who are equally exposed to the causes of the disease; and finds that this risk is between seven and eight times greater for persons above the age of fifty than it is for persons between the ages of fifteen and thirty; hence he suggests the propriety of entrusting the charge of cholera patients to persons in the prime of life. The fourth table is intended to show the influence of previous health on mortality. The fifth table exhibits the manner in which the disease was diffused. The 160 cases came from 134 different vessels; and from each of 122 of these vessels one patient only was admitted. In 1834, when cholera again appeared in London, the *Echo* was fitted up as a cholera hospital, and stationed, as the *Doror* had been, in the river, at Deptford. Thirty-six cases of cholera were received in the *Echo* from the commencement of the epidemic in the early part of August, to its termination in the middle of September. The analysis of these cases shows that the disease was less severe than in 1832; it agrees with the analysis in 1832, as showing the great influence of age on the mortality in cholera, and on the liability to that disease. In 1834 the disease was diffused in the same manner as in 1832; almost all the cases came from different vessels.

CASE OF ANEURYSMAL TUMOUR OF THE ORBIT, CURED BY TYING THE COMMON CAROTID ARTERY.

By GEORGE BUSK, *Surgeon to the Seamen's Hospital Ship, Dreadnought.*

The patient was admitted into the hospital in consequence of severe concussion of the brain, resulting from a blow with the gaff on the right temple, on the 13th of July, 1835, having, at the time, considerable hæmorrhage from the right ear. Sensibility was very slowly recovered, and, on the second day after the accident, he was found to be quite deaf in the right ear. The lids of the left eye were swollen, and its pupil dilated and fixed, neither could the globe be moved in any direction. There was, also, slight paralysis of the facial muscles of the left side. Five days after his admission the pupil of that eye was observed to be elongated downwards, and the vision was slightly impaired. The only motion of the globe was an involuntary rotation on its axis. The paralysis continuing, the eye, after a while, became painful from the constant exposure consequent on the loss of power of the orbicularis; and, on the 31st, the lower half of the cornea had become nebulous, and pus was apparent between its laminæ. On the 20th of November the patient was discharged in consequence of small-pox, and returned, nearly in the same condition as to his other symptoms, on the 1st of December. On the 1st of February, 1836, the author first discovered a pulsating tumour, in the upper and inner part of the orbit, having a decidedly aneurysmal thrill, and proceeded, on the following day, to tie the common carotid artery in the usual place and manner. The pulsation of the tumour and that communicated to the globe immediately ceased, together with all the sounds, internal and external, but returned very perceptibly, though obscurely, four hours afterwards. On the third day, however, no remains of the tumour could be felt, neither were any sounds audible by means of the stethoscope. The case from this time went on very satisfactorily; the ligature separated on the 16th day, and on the 28th of March the patient was discharged from the hospital quite well, with the exception of some remaining paralysis of the face, and continued so when the author saw him about a year ago. The paper concludes with a reference to some other similar cases, one of which, treated by Mr. Guthrie, was verified by dissection; and a postscript is appended to the paper containing a case precisely parallel to that related by the author, which occurred, as a consequence of concussion of the brain, in the London Hospital, in 1834. In this case Mr. Scott placed a ligature on the common carotid with like success.

ON THE SOFTENING OF COAGULATED FIBRINE.

By GEORGE GULLIVER, *Esq., Assistant-surgeon to the Royal Regiment of Horse Guards; communicated by Sir* J. MACGRIGOR, *Bart.*

The author, after remarking the interest attached to inquiries concerning the distinctive characters of various albuminous morbid products, and that the French pathologists have generalised, to a great extent, on the analogy between these substances and

one of the proximate principles of the blood, expresses his opinion that this method of investigation has tended to retard the acquisition of a satisfactory knowledge of them, since, in the present state of pathological science, it is with their specific differences that we especially require to become acquainted; and as pus is the most important of these fluids, from its constituting such a frequent elementary product of disease, an inquiry into the peculiar properties of the various matters commonly comprised under this term appears to be particularly desirable. Hence, in furtherance of this object, he proposes to show that a fluid which has generally been described as purulent, is, notwithstanding, nothing more than coagulated fibrine which has passed into the state of softening, so as to resemble pus in colour and consistency, but differing essentially from it when examined chemically, and by the aid of the microscope. Some experiments are then detailed in which clots of fibrine were submitted to a blood-heat for two or three days in water, from the effects of which concoction the fibrine was reduced to a fluid matter so much resembling pus as not to be distinguished from it, except by examination as above-mentioned, when the colour and consistency of the softened fibrine were found to be owing to innumerable particles, extremely irregular in form and size, instead of to the characteristic globules of pus from which the former also differed in their chemical properties.

Several cases are then related of clots of fibrine more or less softened towards the centre, in the heart, aorta, and veins; such, in short, as have generally been described by authors as instances of suppuration, and accordingly cited as proofs of the transformation of these coagula into pus. From a careful examination, however, of the fluid in these cases, the author finds it destitute of the characters of pus, but possessing all those of softened fibrine, although he does not deny that in certain states of the disease pus globules may occasionally be found, just as they are frequently mixed with the matter of tubercle and cancer.

From the observations of Dr. Davy, as well as his own, the author regards the occlusion of the veins by sanguineous coagula, whether softened or not, as of much more frequent occurrence, and of much more importance in disease, than is generally supposed; and he is led to believe that the affection in question is generally a consequence of the excessive languor of the circulation, and depression of the vital powers, which take place during the last few days of existence in various chronic maladies; and he suggests that the softening of the centre of the clot may possibly be attributable to the circumstance of this point being most distant from the living tissues; for he remarks that, when the organic force is active, fibrine becomes organised or absorbed, or imprisoned, for years, without undergoing much change, in the sac of an aneurysm. In an incidental notice of a repetition of those experiments of M. Gendrin which consist in confining blood in one of its vessels, or in the cellular tissue, and then introducing a foreign body into the clot, the author remarks, in connection with the cause to which the softening of the clot appears to be attributable, that he could never succeed, like M. Gendrin, in producing anything like pus in its substance in healthy animals, although in two instances, in which their vitality had been much exhausted by the effect of previous operations, a fluid possessing all the characters of softened fibrine was found in the coagulum. The author deduces the following conclusions from his experiments and observations:—

1. That coagulated fibrine, when removed from the body, and subjected to a blood-heat, commences to soften in about forty hours, assuming the colour and consistency of pus, but easily distinguishable from it by microscopic and chemical examination.

2. That the purulent-like fluid found in the fibrinous clots of the heart and arteries, and so frequently in the veins, is essentially distinct from pus, and analogous to, if not identical with, softened fibrine.

3. That the softening of coagulated fibrine is an elementary pathological condition of frequent occurrence, distinct from suppuration, and constituting a considerable proportion of the cases generally denominated suppurative phlebitis.

ST. GEORGE'S HOSPITAL.

DISEASES OF THE NERVOUS SYSTEM.

HYSTERIA.

Case 1.—Harriet Coe, aged 22, was admitted under the care of Dr. Seymour, complaining of a severe but dull pain in the chest, which was found not to be increased on inspiration, nor was there much cough or difficult expectoration. Her general health was by no means impaired. The catamenial secretion was frequent and irregular in its occurrence, and she was much troubled with a suffocating globus hystericus. On percussing the superior and inferior regions of the chest, a tumour was discovered at the base of the sternum, appearing to be, in some degree, connected with it, hard in substance, and having a regular undiffused boundary. She was put upon ordinary diet, and ordered to take the following draught twice a day:—

Ammoniated tincture of iron, 1 drachm;
Carbonate of ammonia, 3 grains;
Camphorated mixture, 1½ ounce;
Syrup, ½ drachm. She was also to use a tepid bath every morning.

May 27. She is much the same to-day, and complains of some pain at the pit of the stomach, for the relief of which latter symptom she was ordered,—

Hydrocyanic acid, 3 minims;
Almond emulsion, 1½ ounce. To be taken twice daily.

31. The pains have been relieved by the acid, and she was ordered, in addition, to have a *compound galbanum plaster* applied over the seat of pain, and to see Sir Benjamin Brodie. In the course of two or three weeks her health became visibly improved, the tumour decreased in size, and she was dismissed much better.

PARALYSIS.

Thomas Tanner, aged 40, has had a paralytic attack, about six years since, which was not preceded by a fit, and caused him in walking to turn to the left side. For this he was bled, and was much better in the course of a week. Six weeks after this, he had an apoplectic attack, since which time both motion and sensation in the lower extremities have become visibly impaired. His principal complaint, at present, is from severe pains in the legs and thighs of each side. The pulse is small and weak, and he has no pain in the head whatever. He was put upon broth diet, and ordered to take one drachm of the *ammoniated tincture of guaiacum* three times daily.

May 27. The pains in the limbs are better.

29. Continues to improve. Ordered to take half a pint of the *compound decoction of sarsaparilla* twice daily, with one grain of *opium* at bed-time.

June 3. The pains are better. He is put upon ordinary diet.

5. The pains are greatly relieved, and he is able to walk better.

9. The mouth still continues to be somewhat drawn to the right side, which appears to have been the case to a greater or less degree ever since his first attack of paralysis. He is unable to masticate on the left side of the mouth, and the tongue, when protruded, is drawn to the right side. The pains in the limbs are better.

12. Improving greatly.

14. Very much better in every respect. Ordered to be made an out-patient.

THREATENED PARALYSIS.

Samuel Davis, aged 53, was admitted under the care of Dr. Seymour, and it was reported to the clinical clerk that he had laboured under such symptoms as appeared reasonably to indicate a threatened attack of paralysis. These, it was stated, had continued for upwards of three months with some occasional intermissions. He was ordered to be cupped to the amount of twelve ounces at the nape of the neck, and to be put on fish diet; three grains of *chloride of mercury* were given him every other night, and a *senna draught* on the following morning.

May 25. He complains very much of pains in his legs at night, which Dr. Seymour considers may arise from some latent venereal taint in the constitution, as the patient confesses to having had an "impure acquaintance" about a twelvemonth since. He has no tibial tumours, however, that are perceptible. In order, however, to make "assurance doubly sure" on this point he was ordered to take *extract of sarsaparilla*, one drachm; *decoction of sarsaparilla*, one pint daily; and to take one grain of *opium* every night at bed-time.

31. The pains in the legs are relieved, and he is better in every respect.

June 2. Since our last visit some copper-coloured spots have come out over his body, which he refers to some previous syphilitic eruption, which he now confesses to have had some time since, although he denies ever having taken any "pills." There has been some slight return of head-symptoms, to relieve which, the hospital apothecary ordered him to have a foot-bath of mustard and water every night, with relief to the symptoms which he complained of.

June 9. He is very much improved and complains only of weakness in the lower limbs. The foot-bath to be used only every other night.

12. Sufficiently improved to be made an out-patient.

PARALYSIS OF THE FACIAL NERVES.

Elizabeth Evans has had paralysis of the right side of the face for about a week past; she cannot close the right eye-lid, and the features are much distorted.

Dr. Seymour ordered her to be cupped to twelve ounces at the back of the neck, and to have a blister applied immediately behind the right ear.

Calomel, three grains;
James's powder, three grains, at bed-time; a *senna draught* on the following morning.

May 23. Paralysis not much relieved, some pain in the head, to relieve which she was ordered to lose ten ounces of blood from the right arm.

25. She can close the right eye perfectly, and the general aspect of the countenance is improved. The pulse is weak, and the tongue is tremulous. She complains of much nervous tremor about the muscles of the cheeks. She was ordered to take half a scruple of *assafœtida* immediately, and to repeat it at bed-time, and to have the following draught three times daily,

Compound decoction of aloes, 6 drachms;
Camphor mixture, 6 drachms;

29. She complains of great acidity of stomach, which was ordered to be removed by a *senna draught*.

31. Very little muscular paralysis now remains in the face, but she complains of great pain in the head, and at the pit of the stomach; the pulse is languid, and the tongue tolerably clean. She was ordered ordinary diet, without beer, and to take a *camphor draught*, with fifteen minims of *fœtid spirit of ammonia*.

June 2. She complains to-day of throbbing headach, with ringing in her ears, which was met by the simple exclamation from the physician, "perstet," and one of Dr. Aldis's "P's" was put upon her card.

3. The pulse still keeps weak, and there is a languid anxiety about the countenance, from which, however, all paralysis has disappeared. She still complains of a host of nervous sensations over the epigastric region, to relieve which another "P" was presented.

5. Improving; the eyes act perfectly on the admission of light, and the aspect of the face is natural. Much acid eructation from the stomach, for which a *rhubarb draught*, and "dismissed cured," were written on her card. She accordingly left the hospital on the following day.

HEMIPLEGIA.

Eleanor Melleney, aged 43, was admitted under Dr. Seymour with the following symptoms. She states that about six months since she had very violent pain in the head, which, after a time, was succeeded by the gradual loss of the use of the right side of the body. She was bled for the affection of the head, which gave her very great relief, but she felt no diminution of the paralysis from it. She states that she has never had any fits of any kind whatever. She can, with some assistance, raise the right hand as high as the chest, but no higher. The catamenial secretion is regular; the face is slightly flushed; the tongue furred; the pulse 100, and hard. Dr. Seymour ordered her diet to consist of fish and potatoes, and she was to lose twelve ounces of blood from the arm, and to have a blister applied over the spine, and dressed with *mercurial ointment*.

June 19. The blood drawn was neither buffed nor cupped. She thinks she has some little increase of power over the lower limbs, which, however, is scarcely perceptible. The pulse is quiet, and she complains much of the pain from the blistered surface.

24. The bowels have been confined, for which she was ordered to have a *colocynth pill*, every two hours, until a free discharge from the bowels was induced; the head is very giddy, and the tongue clean.

26. The blister is healed and the bowels have been opened; the giddiness of the head is less, but there appears scarcely any improvement in the original affection.

28. She appears to have mended much within the last few days, and there is some slight power of motion in the lower limbs.

July 3. She is not so well to-day, and the giddiness of the head is worse to-day than it has ever been. The tongue is dry and furred, and the countenance slightly flushed. Another blister was ordered to be applied over the spine.

5. There is double vision of the left eye on directing it inwards. A blister was ordered to be immediately applied over the left temple.

10. There is some little improvement in vision. Another blister was ordered to be applied over the left temple, and its surface dressed with *mercurial ointment*.

13. The vision of the left eye is improved; in every other respect she is the same.

20. Since the last report she has had two more blisters over the left temple, and the power of vision with the left eye as well as the motion in the paralysed limbs is manifestly improved; around the blistered surface there is a slight erysipelatous redness which does not appear disposed to spread. The tongue is clean and the skin cool; the pulse small, and not frequent; the appetite declining.

Aug. 9. At her own request she was allowed to leave the hospital, evidently very much benefitted since her admission.

In making some observations upon this case, Dr. Seymour observed that it was one of paralysis of the right side of the body, of six months standing, succeeding to a painful state of the head. There had been also a paralysed state of the facial muscles of the left side, which had, however, subsided before her admission into the hospital, leaving that side of the face colder than natural. The spine had been blistered, and the blistered surface dressed with strong *mercurial ointment*,—a plan of treatment which, though very painful, was according to his (Dr. Seymour's) experience, more efficacious in such cases than the treatment by *strychnine*, or any other means.

Reception of Dr. Hall's Discoveries in Germany.—To the recent extract from a letter of M. Flourens, Perpetual Secretary of the Institute of France, on the subject of Dr. Marshall Hall's discoveries, the following passage, taken from the "Wissenschaftliche Kritik," may be added:—

After enumerating Dr. Hall's works on the Nervous System, the editor of the "Wissenschaftliche Kritik" observes:—"The contents of these volumes furnish us with a striking proof that in the domain of natural science many great discoveries may be made by deep reasoning and reflections,

new views being opened to us of objects which, although not previously unknown to us as mere external phenomena, were overlooked or neglected, because their true meaning had not been correctly seized. * * * * The beautiful discoveries of Bell, relative to the difference between sentient and motor nerves, had already presented us with an example of this kind, the result of reasoning rather than of observation; but we must say that even these are surpassed by the discoveries of Dr. Marshall Hall, whether we regard the sagacity by which they were made, or the comprehensiveness of their scope, viz., the extensive organic chain of nervous life."

Many words of the original are omitted, to avoid writing hyperbolically; a review of fourteen columns follows. English readers may also consult Stromeyer's "Beitrage," another German authority, on the same subject.

THE ELECTION FOR CORONER.

To the Editor of THE LANCET.

SIR:—Intelligence, long anticipated, has just reached me, that Mr. Adey has at length deemed it an act of policy to relinquish a contest (if contest it could be called) which was altogether futile from the commencement, and which, by a fruitless and stubborn perseverance, could only tend to expose him to the imputation of ungenerous motives.

It is a source of no slight satisfaction to find, that my remarks on the absolute necessity of Coroners possessing adequate medico-legal information, contained in a recent number of THE LANCET, should have by chance immediately preceded an election in the metropolis of England, for the Coronership of the county of Middlesex.

I cannot permit the present opportunity to pass without cordially congratulating you upon your election to the vacant office. The great demonstration in your favour sufficiently shows that the public opinion is undergoing a considerable change, and that medical men will ere long be considered the most competent, and, consequently, the most eligible persons for such appointments.

Let us hope that the very sanguine expectations expressed at a late meeting of the British Medical Association, will be amply borne out, and that the result of this election, which has conferred no trifling distinction on the medical profession, will operate as a successful stimulus, which, spreading throughout the provinces, may in due time terminate in the election of medical coroners throughout the kingdom.

Your persevering exertions to promote the just interests of the medical profession require, as I have before observed, that some general demonstration of acknowledgment should be made on the part of its members; and I perfectly agree with the editor of the "Medical Annual," that the present occasion affords a fitting opportunity for its manifestation. It is a duty, therefore, which medical men owe to themselves, to come forward, and by their subscriptions to the "Medical Coroner's Election Fund," fully prove that your services on their behalf have neither been unappreciated nor unavailing. I am, Sir, your obedient servant,

GEORGE LOWTHER.

Kingston-upon-Hull,
Feb. 23, 1839.

SIR PHILIP CRAMPTON AND SIR —— MARSH.

WE have much pleasure in announcing that Mr. Crampton, who may justly be considered to be the representative of Irish surgery, has been raised to the dignity of Baronet of the United Kingdom. As to Dr. Marsh, whose name is associated with that of Mr. Crampton, in the same Gazette, he is only known to us as a respectable, psalm-singing practitioner.

TO CORRESPONDENTS.

M.R.C.S.L. The breach of etiquette was committed in private, and is beyond the pale of our notice.

Mr. G. Bloomfield. Medical advice is never given to correspondents in the pages of this Journal.

Mr. W. H. Denham still contends, in a reply to the letter of Mr. Muriel which we have received, that the "hospital attendance" of the party whose case has been in debate, "did not commence until October, 1827, and yet that the party was in practice in April, 1829, as a holder of the licence and diploma of the Hall and College." He also asks for "*proof* that the apprenticeship was served." This supplied, satisfactorily, Mr. Denham "will acknowledge the erroneous nature of the information supplied to him."

Mr. Laidlaw's letter received.

We fear that the communication of *Censorius* is a libel.

J. A. S. The information required may be found in the "Medical Almanac" of Mr. Farr, which contains, in addition, a great variety of useful matter.

The correspondence relative to the Fothergillian Medal is under consideration.

Mr. Blake's letter has been received.